THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY, AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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PHILOSOPHICAL MAGAZINE.

I. *On the musical Sounds of the Gases,—on the meteorologic Solution of Water in Air,—Electricity, &c.,—on Water-Pressure Engines for Mines,—on the Confusion arising from various Meanings to the same Words and Marks used in Science.* By Mr. JOHN FAREY.

To Mr. Tilloch.

On the musical Sounds of the Gases.

SIR, As a principal aim of all those who make and communicate to the public, the results of philosophical experiments, must be to have them extensively known, I shall offer no apology for sending you some calculations, on the results of a series of ingenious experiments by Messrs. F. Kirby and Arnold Merrick of Cirencester, on the sounds of an organ-pipe, blown by eight different sorts of gases, compared with the lengths of a sounding string on a monochord, which I find recorded in a late number of Mr. Nicholson's respectable Journal, vol. xxvii. p. 271.

The whole string or *unit* of the monochord was made unison with C, an octave below the tenor cliff, or a fourth below the bass cliff, and thereon—

1st. When *nitrous oxide* (obtained from the nitrate of ammonia) was used, to blow the pipe, in a receiver filled therewith, the length of string was ·112, as I infer, from a mean of four experiments, wherein the highest number was ·115, and lowest ·108: and which therefore (if the whole string vibrated 120 times in a second, see Dr. Rees's Cyclopædia, *Concert Pitch*,) gave 1071·429 vibrations per second, and produced a sound $150\Sigma + 3f + 13m$ *, or a

* See the Table of Intervals in your 28th volume, Plate V. p. 142, by help of which, any other comparisons of these intervals can be made, by simple addition or subtraction.

comma-deficient minor Third, lower than, the sound yielded by the same pipe blown in a similar manner with atmospheric air, as mentioned below.

2d. When *carbonic acid gas* (from marble, &c.) was used, the unison on the monochord was ·108, from a mean of seven experiments, greatest ·117, least ·082; whence the vibrations were 1111·111, and the sound 117·9471Σ + 2f + 10m, or a comma-redundant major Tone + 2·9471 Σ below the sound of atmospheric air.

3d. When *oxygen gas* (from the oxide of manganese) was used, the length was ·095, from a mean of 4, greatest ·100, and least ·83; whence the vibrations were 1263·158, and the sound 4·6761Σ, or less than half a Comma below the sound of atmospheric air.

4th. When *atmospheric air* was used, the length of string was ·0945, from a mean of six experiments, some in and some out of the receiver, whereof the greatest were ·095, and the least ·093; whence the vibrations were 1269·841 per second.

5th. When light *carburetted hydrogen* (from wood) was used, the length was ·089, from a mean of three experiments, greatest ·090, and least ·088; whence the vibrations were 1348·315, and the sound 52·9459Σ + f + 5m, or a Semitone medius + 5·9459Σ + m higher than the sound yielded by atmospheric air.

6th. When *nitrous gas* (from copper and nitric acid) was used, the length was ·089 from a mean of four experiments, greatest ·100, and least ·083; and the vibrations and pitch, as in the last case.

7th. When *ether vapour* was used, the length was ·065, in a single experiment; whence the vibrations were 1846·153, and the sound 330·4717 Σ + 6f + 29m, or a comma-redundant minor Fifth (or 23ds) + 8·4717 Σ + m higher than the sound of atmospheric air.

8th. When *hydrogen gas* (from zinc, &c.) was used, the length on the monochord was ·049, from a mean of eight experiments, greatest ·061, and least ·042; whence the vibrations were 2448·979 in 1^s, and the sound 579·7972Σ + 12f + 50m, or an Octave all but 32·2028Σ + 3m* above the sound yielded by atmospheric air, in the same pipe.

In the Table of Experiments referred to, the states of the barometer

* In an enigmatical Paper in the Philosophical Transactions for 1800, distinguished by its splenetic attacks on the valuable "Harmonics" of Dr. Robert Smith, it is said (see also Nicholson's 4to Journal, vol. v. p. 85) that the pitch of a pipe blown by pure hydrogen gas, *should be* a minor fourteenth (VIII + 7th = 1120Σ + 22f + 97m) higher than if blown by common air!

barometer and thermometer are noted at the time of each experiment, but such have not been taken into my calculations above. In any repetitions of these interesting experiments, it seems to me desirable, that the *pitch* should be ascertained more exactly, by means of the *beats* made with a standard pipe, previously and accurately tuned, by a combination of concords (or by beats) near to the proper pitch: for which previous calculations and preparation the intervals above deduced may prove of some use.

On the Solution of Water in Air, as affecting the Barometer; on Electricity, &c.

Your ingenious correspondent Mr. Richard Walker, at page 376 of your Number for November, in speaking of the application of the barometer as a means of indicating the weather, alludes to the alleged solution of watery vapour in atmospheric air, and mentions "the *dense state* of the air being *fittest for the chemical combination* above mentioned," "a *rare state* of the air being *less capable of receiving the water into chemical combination*;" and yet, lower down in the same page we find it said, that the air "is *incapable* (by having *become colder*) of retaining or suspending it (water) *in a state of chemical combination*;" as though colder air were not more *dense* than warmer air!

In the next page, contrary to the opinion of that veteran meteorologist M. De Luc, we find, that atmospheric electricity is to be considered "rather as a matter of *curious speculation* than of practical utility!" As an antidote to such reasonings and principles of meteorology as these, I beg to refer your readers to an able and original view of these subjects, lately taken by M. De Luc, in Mr. Nicholson's Journal of Natural Philosophy, vol. xxvii. p. 244.

On Water-Pressure Engines for Mines.

I beg to inform Mr. John Taylor, or the gentleman who communicated on the subject of hydraulic pressure-engines for pumping mines, vol. xxxvi. page 394, that it is a mistake to say, that "none have yet been successfully made upon a large scale," because Mr. Trevethick in 1803 erected one in Crash-purse Lead Mine, half a mile south of Yolgrave in Derbyshire, by the fall of 144 feet, of a branch of the Lathkil river, into the famous Hellcar Sough, which has ever since effectually pumped that mine 48 feet below the sough, and enabled large profits to be obtained by the owners, instead of the ruin that had previously attended the concern, as the name implies. And further, that the steady

and effectual operation of this machine, after six years experience, has occasioned another to be lately erected in Bacon-close mine, near the same place.

On the Confusion arising from affixing new and separate Ideas to established Marks or Words.

I cannot, sir, while the pen is in my hand, refrain from noticing the inconsiderate proposal of your correspondent Mr. A. Reirtalp, at page 397 of your last volume, to further bewildering the meaning of the marks ′ ″ ‴, so long and usefully applied to the sexagesimal divisions of the quadrantal arc of a circle: as if it were not enough, that some write ′ and ″ instead of *m* and *s*, to designate minutes and seconds of *time*, &c. and that the reformers of the French denominations of magnitudes have applied °, ′, ″, ‴, to the *decimal* divisions of the quadrant, as well as the names *degree*, *minute*, *second*, &c. which is perhaps the most powerful among the reasons, that this centenary division has not been more adopted.

I am, sir,
Your obedient servant,
Westminster, Nov. 6, 1810. JOHN FAREY.

II. *Of the Koumiss of the Calmucks, and of the ardent Spirit which they distil from Milk. By* EDWARD DANIEL CLARKE, *LL.D.**

EVERY body has heard of the koumiss and the brandy which the Calmucks are said to distil from the milk of mares. The manner of preparing these liquors has been differently related, and, perhaps, is not always the same. They assured us that the brandy was merely distilled from butter-milk. The milk which they collect over night is churned in the morning into butter; and the butter-milk is distilled over a fire made with the dung of their cattle, particularly the dromedary, which makes a steady and clear fire, like peat. But other accounts have been given both of the koumiss and the brandy. It has been usual to confound them, and to consider the koumiss as their appellation for the brandy so obtained. By every information I could gain, not only here, but in many other camps which we afterwards visited, they are different modifications of the same thing, although different liquors; the koumiss being

* From Clarke's Travels in various Countries of Europe, Asia, and Africa, Part I. p. 238, 239, and 259.

a kind

a kind of sour milk, like that so much used by the Laplanders, called *pina*, and which has undergone, in a certain degree, vinous fermentation; and the brandy, an ardent spirit obtained from koumiss by distillation.

In making the koumiss they sometimes employ the milk of cows, but never if mare's milk can be had; as the koumiss from the latter yields three times as much brandy as that made from cow's milk.

The manner of preparing the koumiss is by combining one-sixth part of warm water with any given quantity of warm mare's milk. To this they further add, as a leaven, a little old koumiss, and agitate the mass till fermentation ensues. To produce the vinous fermentation, artificial heat and more agitation are sometimes necessary. This affords what is called koumiss.

They gave us this last beverage in a wooden bowl, calling it *vina*. In their own language it bears the very remarkable appellation of rack and racky, doubtless nearly allied to the names of our East-India spirit, rack and arrack. We brought away a quart bottle of it, and considered it very weak bad brandy, not unlike the common spirit distilled by the Swedes and other northern nations.

Some of their women were busy making it in an adjoining tent.

The simplicity of the operation and of their machinery was very characteristic of the antiquity of this chemical process. Their still was constructed of mud, or very coarse clay; and for the neck of the retort they employed a cane. The receiver was entirely covered by a coating of wet clay. The brandy had already passed over. The woman who had the management of the distillery, wishing us to taste of the spirit, thrust a stick with a small tuft of camel's hair at its end, through the external covering of clay: and thus collecting a small quantity of brandy, she drew out the stick, dropped a portion upon the retort, and, waving the instrument above her head, scattered the remaining liquor in the air. I asked the meaning of this ceremony; and was answered, that it is a religious custom, to give always the first drop of the brandy which they draw from the receiver to their god. The stick was then plunged into the receiver a second time; when more brandy adhering to the camel's hair, she squeezed it into the palm of her dirty greasy hand, and, having tasted the liquor, presented it to our lips..........

We traversed continued *steppes* [immense flats] from Kamenskaia. Camps of Calmucks were often stationed near

the road. We paid visits to several of them. In one of them, containing not more than four tents, we found only women, who were busy in distilling brandy from milk. The women confirmed what we had been before told concerning the materials used for distilling, and said that, having made butter, they were distilling the butter-milk for brandy. We could not credit that brandy might be so obtained; but to prove it, they tapped the still as upon a former occasion, offering us a tuft of camel's hair soaked in brandy, that we might taste, and be convinced.

III. *Proposed Mineral Surveys of the British Counties. Mr.* Kirwan's *Opinions on this Subject.*

It appears to have been the original intention of the President and Members of the Board of Agriculture to ascertain,

"1. The riches to be obtained from the surface of the national territory.
2. The mineral or subterraneous treasures of which the country is possessed.
3. The wealth to be derived from its streams, rivers, canals, inland navigations, coasts, and fisheries; and
4. The means of promoting the improvement of the people in regard to their health, industry and morals, founded on a *statistical* survey, or minute and careful inquiry into the actual state of every parochial district in the kingdom, and the circumstances of its inhabitants."

Conceiving, that under one or other of these heads, every point of real importance, that can tend to promote the general happiness of a great nation, will be included.

The first point, viz. the *cultivation of the surface*, and the resources to be derived from it, appearing to have a prior claim on the attention of the Board, it has been particularly kept in view in the selection of their surveyors or reporters, and in the instructions given to these gentlemen, who have done themselves so much credit, and the country so much service, by the many able county reports which they have enabled the Board to present to the public; in which reports, the three latter subjects are only incidentally touched upon, although much valuable matter has been collected and published relating to them, on the two latter heads in particular. The second head, that of *mineral surveys*, appeared of such a distinct nature, and of so much importance, to be entered on, when the agricultural part of their surveys was

was accomplished, (which appears now to be almost the case) that the Board consulted several eminent mineralogists, as to the proper heads of inquiry, and on the best manner of conducting mineral county surveys; among whom was Mr. Richard Kirwan, and whose reply to the inquiries of the President on these heads is printed in the first volume of their "Communications," appendix lxviii.

I conceive that I shall be forwarding the very important objects of the Board, in giving a place to this letter of Mr. Kirwan. EDITOR.

"SIR,—WHEN last I had the honour of meeting you in London, you were pleased to express a wish, that in order to promote a mineralogical survey of the kingdom, the proper *objects of inquiry* in every county should be briefly pointed out, and in such a manner as that they may easily be discerned, even by such persons as cannot be supposed to be deeply versed in such matters. In compliance with your wishes, I now send you the principal heads of such inquiries, many of which may be satisfactorily answered by architects, common land surveyors, masons, and even common labourers; though it were to be wished that persons of still superior education, as those of the clerical and medical professions, were also consulted. In the mining counties in particular, as Cornwall, Derbyshire, Flintshire, Devonshire, Lancashire, &c. the overseers of the works could give the best information; in many, several private gentlemen are known to be well instructed in those matters.

1st. Are there any mountains in that county? and their names? what is their course or direction? their height, known or reputed? whether of steep or gentle ascent, cultivated, woody, or otherwise? dry, or abounding in springs? in continued ridges, or separate and distinct? what sort of stone are they formed of (whether at the top, middle or bottom? what is their inclination) or is found in them? as granite, sandstone, whin, limestone, marble, alabaster, Derbyshire-spar, freestone, flags, slates, gravel, flint, &c., are they solid or cavernous? are they single, or one within the other? and the different stones in each? What other hills or eminences are there? of what composed? stony or chalky, and their direction and bearings, with respect to the mountains?

2dly. What rivers traverse the county? their names, length, breadth, depth, and direction? what lesser streams flow into them, and their direction? temporary or perennial, apt

apt to overflow, or otherwise? what lakes, and their extent?

3dly. What, or do any, mineral springs occur, in any or what parishes, and what is their nature, if known? by whom examined, and when?

4thly. What sort of stone commonly occurs in different parishes, in the plains, in sand-pits, making of roads, for buildings, mill-stones, &c., and what quarries?

5thly. What are the different soils that occur in different parishes, as clays, marles, sands, loams? are these last clayey, or sandy, or chalky? what mosses? do they yield peat? have brick-clay, fire-clay, potters-clay, terras, fuller's-earths, tripoli, &c. been found?

6thly. What mines are found in the country? whether metallic, or saline, or coaly? whether in veins, beds or bellies? their inclination and direction, or bearing? how is the ore raised? where are the ores worked, and the general mode of smelting them? were any, and what improvements lately made in the manner of draining them, freeing them from water, working or smelting them? are there any still required? have all the hills and mountains been yet examined, with regard to their mineral contents?

7thly. Have any collections or cabinets of the fossils of each county as yet been formed? let samples of each, docketed, with the name of the place in which it was found, be sent.

Much information, relative to these inquiries, may be found in the Philosophical Transactions, histories of particular shires and counties, as Cornwall, Derbyshire, &c.; tours through England, Wales, and Scotland, the works of Woodward.

A committee may perhaps be named, to examine these and arrange the particulars found in each shire, which might perhaps help the persons, to whom queries are to be addressed, to recollect and find them.

These are the particulars which at present occur to me: they may be digested, enlarged, and perhaps better methodized, in a committee. When answers are had, which at furthest may be in a year or two, a mineralogical map of the kingdom may be formed, serving as a fund of information, and a ground of future improvements in agriculture, commerce, and all the arts. I have the honour to be

Your very obedient and humble servant,

Dublin, Nov. 17, 1795. R. KIRWAN."

To Sir John Sinclair, Bart. &c. &c.

IV. *Memoir*

IV. *Memoir on the Diminution of the Obliquity of the Ecliptic, as resulting from ancient Observations.* By *M.* LAPLACE. *Translated from the* "Connoissance des Tems for 1811" *by* THOMAS FIRMINGER, *Esq.*

[Concluded from vol. xxxvi. p. 434.]

Greek Observations.

PYTHEAS' observation at Marseilles was made between the two epochas of the preceding observations. In the second book of his Geography, and the 4th chapter, Strabo says: "According to Hyppacus, at Byzantium, the proportion of the gnomon's shadow is the same as Pytheas pretends to have observed at Marseilles;" and in the 5th chapter of the same book he adds, "at Byzantium, in the summer solstice, the proportion of the gnomon's shadow is $42\frac{1}{5}$ to 120."

It was no doubt from that observation that Ptolemy, in his Almagestes (b. xii. ch. vi.), draws through Marseilles the 14th parallel, in which the shadow's length at the summer solstice is $20\frac{5}{6}$ parts, the gnomon's being 60 parts. Pytheas was, at latest, contemporary with Aristotle; therefore his observation may, without any sensible error, be referred to the year 350 before our æra. After correcting it with the refraction and parallax, it gives $19°\ 28'\ 29''$ for the distance of the sun's centre at the solstice, to the zenith of Marseilles. The latitude of that town's observatory is $43°\ 17'\ 49''$, from which if the preceding distance is subtracted, we shall have $23°\ 49'\ 20''$ for the ecliptic's obliquity in Pytheas' time.

The new Solar Tables, published by the Board of Longitude, which are founded on the formulas on book vi. chap. xii. of *Méc. Cél.* give $23°\ 46'\ 7''$ for the ecliptic's obliquity, corresponding to the year 350 before our æra; the difference $3'\ 13''$ is within the limits of errors of which Pytheas' observation is susceptible.

About a century later than Pytheas' observation, Eratosthenes undertook to measure the earth, and founded that measurement on solstitial observations of the gnomon made at Syena and Alexandria. (Cleomedes, b. i. On contemplation of Celestial Bodies, ch. x. Of the Earth's Magnitude.) Eratosthenes made use of a vertical gnomon raised in a spherical segment. The summit of the gnomon being in the centre of the segment, he found the distance between the zeniths of Syena and Alexandria to be equal to a 50th part

part of the circumference. Thus, according to this astronomer, the sun being at the zenith of Syena at the summer solstice, he found on the same day its distance from the zenith of Alexandria 7° 12′. This distance was from the upper edge of the sun; for the ancient astronomers did not correct the sun's altitude as observed by the gnomon, to obtain that of the sun's centre: which is the reason why their latitudes were too small by the apparent diameter of the sun. This is evident for Alexandria, whose latitude is calculated by Ptolomy at 30° 58′; while, according to Nouet's observations, it is 31° 13′ 15″; greater therefore by 15′ 5″, which is nearly the sun's semi-diameter. The apparent altitude of the sun at the summer solstice at Alexandria, as observed by Eratosthenes, must then be corrected by the sun's semi-diameter, the refraction and parallax; which gives 7° 27′ 50″ for the distance from the sun's centre to the zenith of Alexandria at the said solstice. Subtracting it from the latitude of Alexandria, as observed by Nouet, the difference 23° 45′ 7″ will be the obliquity of the ecliptic in Eratosthenes's time, or towards the year 250 before our æra. According to the formulas of *Méc. Cél.* it was at that epocha 23° 45′ 19″, which agrees remarkably well with Eratosthenes's observations. These observations, together with Pytheas' and the preceding Chinese, all combine therefore to show that the ecliptic's obliquity, previous to our æra, was very nearly the same as is given by the formulas of *Méc. Cél.* Let us now consider the observations made since the commencement of our æra.

OF ANCIENT OBSERVATIONS POSTERIOR TO OUR ÆRA.

Chinese Observations.

The first of these observations dates from the year 173 of our æra. It is recorded as follows, in Father Gaubil's MS. (*Conn. des Tems* for 1809, p 395.)

"On the 9th of October 173, at Loyang, meridian shadow ten feet. On the 7th of February 174, meridian shadow nine feet six inches. These shadows were observed carefully."

The gnomon was eight feet long.

The altitude of the sun's centre resulting from the first shadow is 38° 22′ 15″, after being corrected by refraction and parallax. The one resulting from the later shadow, also corrected, is 39° 31′ 9″,4. Let x be the altitude of the equator at Loyang. If we calculate from the new Tables of the Sun just published by the Board of Longitude, the

sun's

sun's declination for the 9th of November 173, at noon, at Loyang, which is 7° 20′ 6″ further east than Paris; and if we multiply afterwards by y the variation of declination corresponding to 10′ of increase in the situation of the sun; and, finally, if we design by z an increase in the obliquity of the ecliptic;—we shall have the two equations,

$$x - 16° \; 56' \; 58'',9 - y \cdot 2' \; 53'',1 - z \cdot 0,69121 = 38° \; 22' \; 14'',0$$
$$x - 15 \;\; 37 \;\; 44 \;,3 + y \cdot 3 \;\; 5 \;,9 - z \cdot 0,63720 = 39 \;\; 31 \;\; 9 \;,4$$

which two equations give

$$x = 55° \; 14' \; 14'',4 + z \cdot 0,66668 \, ;$$

and therefore the latitude of Loyang resulting from these observations is $34° \; 45' \; 45'',6 - \frac{2}{3}z$.

By a mean between F.F. Regis and Mailla's observations, this latitude is 34° 46′ 15″: and it has been seen that this result differs little from what is given by Tcheou-Kong's observations: thus we have

$$-\tfrac{2}{3}z = 29'',4 \, ;$$

which gives $z = -44'',1$. The ecliptic's obliquity given by the quoted tables was at that period 13° 54″ greater than in 1750. The preceding observations give therefore an increase of 13′ 9″,9 in that obliquity, differing very little from the result of the formulas of *Méc. Cél.* on which these tables are founded. In order to admit an invariable obliquity, z should be made $= -13' \; 54''$; in which case we should have 34° 55′ 1″,6 for Loyang's latitude, which cannot be admitted.

Supposing $z = 0$ in the preceding equations, we shall have

$$y = -1,7242.$$

The preceding observations, therefore, appear to indicate a diminution of 17′ in the sun's place, as indicated by the tables. This diminution is too considerable to be admissible, and it is more natural to attribute it to errors in observations. Supposing y and $z = 0$, we shall have by the first observation 34° 40′ 47″ for the pole's altitude, and 34° 51′ 6″,3 by the second; which gives a mean of 34° 45′ 56″,7 for that altitude.

In the year 461, Tsou-tchong, a learned Chinese astronomer, determined the instant of the winter solstice. This is what is related on that subject in the MS. quoted by Father Gaubil. (*Conn. des Tems* for 1809, p. 389.)

"This solstice was determined at Nankin, the year Sin-tcheou, 5th of Tamin, on the day Y-yeou, 31 ke after midnight; which answers to the year 461, 20th December, $7^h \; 26' \; 24''$ of the morning.

"The

"The astronomer Tsou-tchong determined this solstice, and it is the first Chinese solstice the determination of which we find detailed. Here it is: On the day Gin-su of the 10th moon, noon shadow ten feet seven inches seven fen five li. On the day Ting-ouey of the 11th moon, noon shadow ten feet eight inches one fen seven li five hao. On the day Vou-chin, 11th moon, noon shadow ten feet seven inches five fen two or three li.

"The first of January 462 was Ting-yeou; therefore Gin-su was the 27th of November 461: Ting-ouey the 11th of January 462, and Vou-ching the 12th of January 462.

"Tsou-tchong examined the difference in the shadows of the 11th and 12th of January, and by the rule of three that he uses, he found between the 11th and 12th of January, the moment when the noon shadow was equal to the noon shadow of 27th of November: he computed the days ke, fen, between that moment and the noon of 27th of November: he halved it, which he added to the noon of the 27th of November, and he thus found this solstice to be the 30th of December at 31 ke after midnight, or $7^h\ 26'\ 24''$ of the morning. Until the Jesuits' arrival, the Chinese astronomers have made use of this method to ascertain the solstices.

"The gnomon's length was eight feet:—the foot is ten inches; the inch, ten li; the li, ten hao.

"Tsou-tchong took great precautions to have the gnomon perpendicular. The plane was levelled, and he measured exactly the shadow; he wanted to correct the errors of Hoching-tien's method. According to that method, the solstice of the year 461 should have taken place on the day Kia-ching (19th December), $7^h\ 12'$ afternoon. The solar year of Hoching-tien was 365 days 24 ke $60'\ 71''$, or $5^h\ 53'\ 44''$. Tsou-tchong undertook to show the errors of this computation, and said that the solar year was of 365 days $5^h\ 49'\ 40''$. He does not say upon what observations he founded this determination. This author also corrected the time of the solstice of the year 173; which he determined on the 22d of December, at $9^h\ 7'$ of the morning."

The observations on which Tsou tchong founded his determination are evidently this solstice of 173, and the one he determined in 461; for the interval of these two solstices, such as Tsou-tchong has determined them, is of 288 solar revolutions, and of 288 Julian years, wanting two days one hour $40'\ 36''$; which gives for the year's length 365 days $5^h\ 49'\ 39''$, the same within $1''$ as Tsou-tchong's.

If

If x is called the altitude of the equator at Nankin; y the number into which the variation of the declination is to be multiplied, corresponding with 10′ of increase in the sun's longitude; lastly, if z design an increase in the obliquity of the ecliptic,—Tsou-tchong's observations give us the following equations:

$$x - 21^\circ\ 39'\ 59'' - y \cdot 1'\ 41{,}0 - z \cdot 0{,}90669 = 36^\circ\ 18'\ 51''$$
$$x - 21\ \ 42\ \ 48{,}5 + y \cdot 1\ \ 41{,}0 - z \cdot 0{,}90678 = 36\ \ 12\ \ 48$$
$$x - 21\ \ 39\ \ 25{,}7 + y \cdot 1\ \ 44\ \ 7 - z \cdot 0{,}90093 = 36\ \ 21\ \ 21$$

which three equations give $x = 57^\circ\ 56'\ 55''$, $+\ z \cdot 0{,}90527$, giving the latitude of Nankin $= 32^\circ\ 3'\ 5'' - z \cdot 090527$.

According to Father Fontaney's observation, the latitude of Nankin is $32^\circ\ 4'$: comparing it with the preceding, we shall have $z = -1'\ 0'',7$; but the observations being susceptible of an error of 1′, and the town of Nankin being so very extensive that the difference of latitude of its extreme points is much greater, these observations may be considered as agreeing with the formulas of *Méc. Cél.* The ecliptic's obliquity as given by the formulas of *Méch. Cél.* was then $23^\circ\ 39'\ 8'',9$; it was then, by Tsou-tchong's observation, $23^\circ\ 38'\ 8'',2$. Supposing $z = 0$, we shall have

$$y = -1{,}12850,\ x = 57^\circ\ 56'\ 8''.$$

This value of x differs little from the preceding. The value of y seems to indicate, like that of the preceding observation, a diminution in the sun's longitude as given by the Tables. But the modern observations do not admit of that diminution. However it be, Tsou-tchong's observations deserve so much the more confidence, as that experienced observer's intention having been to correct the errors of his predecessors, he took special care to make them accurately. Besides, it was to the solstice determined by those observations that Cocheou-king has compared his own observations, in order to obtain the year's length, which he found of 365 days 2425, the same exactly as our Gregorian year.

We find that in the year 629, an observation was made carefully by an experienced astronomer, with the intention of correcting an error of his predecessors. "It has been seen in the second observation," says Father Gaubil, (*Conn. des Tems* for 1809, p. 357,) "that Litchun-foung had objected against the solstitial shadows improperly applied to Siganfou. This astronomer was therefore desirous of observing exactly the noon shadow of the solstices at Siganfou with an eight-foot gnomon. He made his observation the year Ki-tcheou (629) of Tching-Koan's empire in the town of Tchang-gou, or Siganfou,

Siganfou, the capital of the empire. On the day Kouey-hang of the 5th moon (19th June) was the summer solstice; the noon shadow was one foot four inches six fen.

"On the day Ping-you of the 11th moon (19th Dec.) was the winter solstice; the noon shadow was twelve feet six inches three fen."

The summer shadow gives for the altitude of the sun's centre, corrected by the refraction and parallax, 79° 23′ 31″,6.

The winter shadow gives for the same altitude, corrected in like manner, 32° 3′ 21″,3.

The difference between these two latitudes is 47° 20′ 10″,3, of which the half, 23° 40′ 5″,1, is the ecliptic's obliquity as determined by these observations. According to the new Tables founded on the formulas of *Méc. Cél.* the ecliptic's obliquity should have been at that period 23° 38′ 7″. The difference is not very considerable, considering the uncertainty of the observations.

The pole's altitude at Siganfou resulting therefrom is 34° 16′ 33″,5. This altitude has been observed to be 34° 16′,0 by the Jesuits' missionaries. This agreement is a proof of the justness of the observations.

I am at last arrived at the numerous and exact observations of the greatest astronomer China has to boast of, Cocheou-King. No observer before him has left such accurate observations as his. Their accuracy is even greater than that of the observation of Tycho: it is owing to the magnitude of the instrument he made use of, and to the precautions he took to ascertain its adjustment. I shall first relate the observations of the quoted MS. (*Conn. des Tems* for 1809, p. 392.)

"Winter solstice at Peking. This solstice is marked for the year Ting-tcheou of the empire of Cobilay (1277), at 7ʰ 43′ of the morning of the day Kouey mao (14th Dec.)

"Summer solstice at Peking, in the year Vou-yu of Cobilay's empire (1278), at 10ʰ 43′ 12″ in the morning of the day Y-se of the 5th moon (14th June).

"Winter solstice at Peking, for the year Ky-mao of Cobilay's (1279), 4ʰ 33′ 36″ of the morning of the day Sin-hao of the 5th moon (15th June.)

"Winter solstice at Peking, of the year Ky-mao of Cobilay (1279), 7ʰ 28′ 48″ at night, of the day Kouey-tcheou (14th December) of the 11th moon.

"Winter solstice at Peking, of Kentchin, of Cobilay (1280), 1ʰ 26′ 24″ after midnight of the day Ky-ouey of the 11th moon (14th December.)

"These

"These solstices were determined by Cocheou-King, according to Tsou-tchong's method, as before related. Tsou-tchong only made use of three observations; his gnomon was eight feet long. Cocheou-King made use of seven, eight, nine, ten corresponding observations; and his gnomon was 40 feet long.—The last solstice in the epocha of Cobilay's Astronomy, as set right by Cocheou-King.

"The solstices deserve to be examined, because of the noon shadow observed by Cocheou-King with that gnomon. (*Conn. des Tems* for 1809, p. 399.) He made a small aperture in a sheet of copper to transmit the sun's image. This aperture, says he, was like that of a needle; and from the centre of that hole it was that he took the height of the gnomon, and he measured the shadow as far as the centre of the image. Till then, says he, eight-foot gnomons only were used, and by their means the upper edge of the sun only was observed: it was difficult (he adds) to distinguish the edge of the shadow, and the eight-foot gnomon was too short. These are the reasons (continues Cocheou-King) that have induced me to make use of a gnomon forty feet long, and to take the image of the sun's centre.

"At Pekin, in the summer solstice, with a forty-foot gnomon, meridian shadow of the sun's centre eleven feet seven inches; in the winter solstice, 79 feet eight inches.

"It was in the years 1277, -78, -79, and -80, that Cocheou-King made these observations, and considering the pains he took for levelling and the ascertaining of the measures, they appear exact.

"In the year 1279, and on the day Y-ouey of the 2d moon (31st March), noon shadow of the sun's centre 26 feet three inches four li five hao.

	Feet.	Inch.	Fen.	Li.	Hao.
"On the 16th March, noon shadow	32	1	9	5	5
"On the 29th August, .. do. ..	25	8	9	9	0
"On the 29th June, .. do. ..	12	2	6	4	0
"On the 29th November, do. ..	76	7	4	0	0
"In the year 1278, 10th June, do.	11	7	7	7	5

"There were many other noon shadows taken by means of that forty-foot gnomon. If they are wished for, they will be detailed."

It is much to be regretted, on account of the exactness of these observations, that they were not given in a greater number; and that Father Gaubil's offer has produced no effect. It is very desirable that the learned and missionaries in China, who are able to obtain them, may be invited to make them known to us, and to give us, on the subject of

of Chinese astronomy, and particularly of Cocheou-King's, all the details they will be able to get at.

Let us first discuss such of the gnomon's observations as were not made on the solstices. These observations reduced to feet, containing a great number of decimals, appear to have been made, or at least set down, with more precision than those of the solstices.

The three noon observations made about the solstices are those of the 10th June 1278, 29th June 1279, and 29th Nov. 1279. The corresponding lengths of shadow observed were

11 feet 7775; 12 ft. 264; 76 ft. 74,

which give for the corresponding distance of the sun from the zenith, corrected by refraction and parallax and reduced to the solstice,

16° 20′ 35″,6; 16° 20′ 38″,9; 63° 24′ 57″.

The mean of the two first observations gives 16° 20′ 37″,2 for the sun's distance from the zenith in the summer solstice; which being subtracted from the sun's distance from the zenith in the winter solstice, half the difference will give 23° 32′ 9″,9 for the apparent obliquity of the ecliptic. The nutation was then − 7″,4; so that the true obliquity was 23° 32′ 25″ in 1279. According to the new Tables, this obliquity should have been 23° 32′ 22″,5; the difference 20″ is within the limits of errors in observations. Half the sum of the two distances of the sun to the zenith gives 39° 52′ 47″,1 for the apparent distance of the equator from the zenith, or for the apparent altitude of the pole. Subtracting 7″,4 on account of the nutation, about the middle of 1279, we shall have 39° 52′ 39″,7 for the true altitude of the pole.

The two lengths of solstitial shadows, eleven feet seven inches, and 79 feet eight inches, give for the respective distances of the sun from the zenith, after being corrected for the refraction and parallax,

16° 18′ 28″,9, and 63° 24′ 24″,

whence is found 23° 32′ 57″,5 for the ecliptic's obliquity, and 39° 51′ 26″,5 for the altitude of the pole. These results differ a little from the preceding; but it may be supposed with some probability that the difference is owing to some decimal that has been omitted in the solstitial lengths of the shadows, in which it appears that the first decimal only was preserved.

Considering now the observations made towards the equinoxes; that is to say, those made on the 15th March, 30th March, and 29th August, the lengths of the observed shadows

shadows give for the apparent distances of the sun's centre from the zenith, after being corrected for the refraction and parallax,

38° 50′ 27″,4 ; 33° 4′ 0″,5 ; 32° 55′ 48″,5.

Taking, therefore, 39° 52′ 47″,1 for the apparent distance of the equator from the zenith, we shall have the three north declinations of the sun following:

1° 2′ 19″,7; 6° 48′ 46″6; 6° 56′ 58″,6;

which gives for the apparent longitudes of the sun,

2° 36′ 5″,2; 17° 16′ 36″,7; 5ˢ,12° 22′ 1″,3.

These longitudes, calculated according to the new Tables, are,

2° 35′ 10″,6; 17° 17′ 24″,1 ; 5ˢ 12° 23′ 5″,0.

The errors of the Tables are therefore,

− 54″,6, + 47″,7 + 1′ 3″,7,

which are within the limits of errors of observations.

These observations are very fit to determine the equation of the sun's centre at their epocha. They give that equation greater by 122″ than in 1800; and thereby confirm evidently its successive diminution, in the same manner that the observations made about the solstices confirm the successive diminution of the ecliptic's obliquity.

It remains for us to consider Cocheou-King's observations of the solstices. Calculating from the new Tables the sun's longitude at the moments of the solstices, we have the following results:

	Longitude of the Sun.	*Error of the Tables.*
1277, 14th December,	8ˢ 29° 59′ 41″,8	−18,2
1278, 14th June,	3 0 2 14,2	+2′14,2
1278, 14th December,	8 29 59 45	−15,0
1279, 14th June,	3 0 2 24	+2′24,0
1279, 14th December,	9 0 0 19,1	+19,1
1280, 14th December,	9 0 0 35,8	+35,8

The errors are very inconsiderable; but it ought to be remarked that they are greater in the summer solstices, and almost nothing in the winter. This difference may be explained by observing that Cocheou-King determined the moment of each solstice by means of a great many meridian lengths, before and after the solstice. Supposing then that he has chosen some observations nearest the equinoxes, a time when the daily variation of the declination is considerable, this astronomer supposed the great axis of the sun's orb perpendicular to the line of equinoxes, as it is seen in the abridged History of Chinese Astronomy; and in 1280 the apogæum was advanced 3ˢ 34′, according to the new Tables. Cocheou-King was thus wrong in

fixing the moment of the solstice at the middle of the interval of time elapsed between the two equinoxes, or between the two moments when the two shadows before and after the solstice were equal. It is easy to perceive that he fixed for the summer solstice half an hour too late; and for the winter, as much too soon; so that the errors of the Tables at the summer solstice should exceed those of the winter solstice by about 2′ 27″. This is in fact the case, very nearly, in the preceding observations. The mean error in the preceding observations is for the winter solstice 5″,4, for the summer, 2′ 19″1: thus the mean error of the six preceding observations is 1′ 12″,5: dividing it by 521, the number of years elapsed from 1279 to 1800, the quotient 14″ will be the amount of what the secular motion of the sun should be increased, according to the preceding solstices; which would diminish the year's length about 3″,5. The above-related observations of lengths of shadows about the equinoxes give —3″,6 for the mean error of the Tables, after the spring equinox, and + 1′ 3″,7 for the error before the autumn's equinox: so that the mean error of the Tables given by these observations is 60″,1: and it should be remarked that this error is independent of the one that may have been committed in the equator's situation; from which results an increase of about 11° in the secular motion of the sun. However it be, the smallness of these errors proves the goodness of the observations, and makes us regret that we have not a greater number.

We see in the History of Chinese Astronomy of Father Gaubil, published by Father Sauciet, p. 72, 3d part, that in the winter solstice of the year 1280, Cocheou-King determined the sun's position in the constellation Hiu of 315°,1075 Chinese, that is to say, distant 321°,1075 from the commencement of the constellation Hiu. This constellation commences at the star β of Aquarius; so that the solstice was, according to Cocheou-King, distant from that star 321°,1075 Chinese. This astronomer, and generally all the Chinese astronomers until the Jesuits' arrival, have divided the circumference into degrees, so that each degree represented the mean daily motion of the sun. This degree thus varied like the duration which they gave to the solar year; and Cocheou-King made it 365 days 2425, which reduces the 321°,1075 Chinese to 316° 29′ 58″ sexagesimal. The longitude of β of Aquarius for the 1st January 1281, and calculated by the formulas of vol. iii. of my *Méc. Cél.* is 10ˢ 30° 2′ 15″; the winter solstice was then distant 346° 35′ 45″; Cocheou-King's error was therefore only 5′ 47, which is very inconsiderable.

ARAB-

ARAB AND PERSIAN OBSERVATIONS.

Mr. Caupin has been so good, at my desire, as to translate the part of Ebn-Jounis' works which contains the Arabian observations. His translation is published in the 7th volume of Notices of MSS. It contains the most numerous collection of Arabian observations; and among them, there are several relative to the ecliptic's obliquity. There we see that in the year 214 of the Hegira, the astronomers of Almamon have observed at Bagdad the obliquity of the ecliptic 23° 33′, and that three years after they observed it at Damas 23° 33′ 52″. However, Ebn-Jounis relates the following passage, according to Ebn-haten-Aluvirizi.

"The obliquity of the ecliptic of the astronomers of Almamon is the same as still continues in our times. It was observed by them with much exactness; and though they have not equally succeeded in their observations, owing to the knowledge they wanted, this last has been very well made, on account of the magnitude and the goodness of the instrument, and of the little difficulty of the operation, and of the little help they had. This obliquity is 23° 35′." The Arabian astronomers appear to have generally confined themselves to that determination; but I find no other detailed observations but those of Albatenius and Ebn-Jounis. In his work *de Scientia Stellarum*, ch. iv. Albatenius says: "With an instrument formed of several sides and a very long cross staff, such as is described by Ptolomy in his Almagestes, after having ascertained the instrument's position as well as could be, I found in the town of Arache, that the smallest meridian distance of the sun from the zenith was 12° 26′; and that the greatest was 59° 36′. These two distances, corrected by the refraction and parallax, become 12° 26′ 10″, and 59° 37 32″. Half their difference gives 23° 35′ 41″ for the ecliptic's obliquity, at the time of Albatenius; that is to say, about the year 881. The formulas of *Méc. Cél.* give for that epocha 23° 35′ 13″, which agrees remarkably well with Albatenius's observation."

Here now is Ebn-Jounis' observation, as extracted from ch. xi. of his work.

"I have measured the greatest declination, and find it 23° 35′, by making the parallax of the sun different from that given by Ptolomy, as I shall explain in this Table. I have found by the instruments of our Lord the Prince of the Faithful, Alaziz-Bellah-Nazar-Aboulmanzer, the sun's altitude at noon, after being corrected by the parallax, which

diminishes it to 36° 21′ 30″, the sun then being in the first degree of Capricorn. I have taken this altitude with all the accuracy possible; I have also found for the altitude, corrected by the parallax in the beginning of Cancer, and when at its maximum, 83° 31′ 30″. Subtracting from the greater the less of the two altitudes, we have 47° 10′, the half of which, or greater declination, is 23° 35′, which is what I have adopted in this Table. I have also compared a great many times the meridian altitudes at the beginning of Cancer and Capricorn, with the corresponding altitudes before and after noon; I have found them to agree with the greatest declination I have observed: therefore, I can warrant its exactness.

"I have chosen those two points of the ecliptic for this research, because, if there was an error in the sun's place of even several minutes, that would not produce any sensible difference, the change of declination being at that time very small."

Though the author says that he has employed a different parallax of the sun from Ptolomy's, which is not indicated in the part of the work we are in possession of, every thing induces us to believe, however, that the difference is very inconsiderable. We may therefore adopt here, without any sensible error, the parallax of Ptolomy to re-establish Ebn-Jounis' observations to what his instrument has given. This parallax is 2′ 51″, and becomes 2′ 18″ at 36° 21′ 30″ of altitude; and thus the smallest meridian altitude observed by Ebn-Jounis was 36° 19′ 12″. Subtracting from it 1′ 19″, on account of refraction, and adding to it 7″ for the parallax, such as is given by modern observation, we shall have 36° 18′ 0″ for the smallest real altitude of the sun. To correct likewise the observation of the greatest altitude, we must subtract 18″ on account of the false parallax, and 7″ for refraction: we must besides increase it one second on account of the true parallax, which gives 83° 31′ 6″ for the altitude thus corrected. Halving the difference of the two latitudes, we have 23° 36′ 33″ for the ecliptic's obliquity at Ebn-Jounis' time; that is to say, about the year 1000. Half their sum gives 30° 5′ 27″ for the latitude of Cairo.

This latitude has been found 30° 3′ 20″ by the French astronomers in the house of the Institute, situated at a very small distance from the spot where it is presumed, with much likelihood, that the Arabian astronomer made his observation. Making use of the French observation, and comparing it with the greatest altitude of the sun, as determined by Ebn-Jounis, we shall obtain an obliquity more exact,

exact, more independent of the false parallax he attributed to the sun's refraction, errors from division of the instruments, and of those of the observations; the latitude 30° 3′ 20″ added to 83° 31′ 6″, gives 113° 34′ 26″; from which if we subtract 90°, we shall have 23° 34′ 26″ for the obliquity of the ecliptic towards the year 1000. The formulas of *Méc. Cél.* give 23° 34′ 50″, which agrees, as nearly as can be desired, with Ebn-Jounis' observations.

The Persian astronomy presents a detailed observation of the obliquity of the ecliptic, made by Ulugbey, in 1437, with a great instrument, which probably was a gnomon of a very great length. This great observer found at Samarkand, the capital of his dominion, the sun's altitude at both solstices, corrected by the parallax which he supposed the sun to have, equal to 73° 52′ 54″ for the summer solstice, 26° 52′ 20″ for the winter's. He made the parallax of the sun 2′ 29″,4: the altitudes, as he has observed them, were then 73° 52′ 12″,5, and 26° 56′ 6′,7. Correcting them by refraction and true parallax, they become 73° 51′ 58″,4 and 26° 48′ 22″,6, which gives 23° 31′ 48″ for the obliquity of the ecliptic in 1437, and 39° 39′ 49″ for the latitude of Samarkand. According to the formulas of *Méc. Cél.*, the obliquity of the ecliptic, at that epoch, should have been 23° 31′ 5″, which only differs 43″ from the result of Ulugbey's observations.

Let us now collect the results we have just found:

OBSERVATIONS ANTERIOR TO OUR ÆRA.

		Obliq. of Ecliptic by Observation.		*Obliquity by Formulas.*		*Excess of the 1st over the last.*
1100,	Cheou-King	23° 54′ 22″,0	..	23° 51′ 58″	..	2′ 4″,1
350,	Pytheas ..	23 49 20	..	23 47 7	..	3 13
850,	Eratosthenes	23 45 39	..	23 45 19	..	0 12
50,	Lieou-hiang	23 45 39	..	23 44 3,4	..	1 34,6

OBSERVATIONS POSTERIOR TO OUR ÆRA.

173,	Obser. Chinese	23° 41′ 33″	..	23° 42′ 17″	..	44″,1
461,	Tsou-chong ..	23 38 52,3	..	23 39 52	..	1′0,7
629,	Litchoufoung..	23 40 4,1	..	23 38 17	..	1′47,
880,	Albatenius ..	23 35 41	..	23 35 13	..	28
1000,	Ebn-Jounis ..	23 34 26	..	23 34 50	..	24
1279,	Cocheou-King	23 32 2,4	..	23 32 22,5	..	20
1437,	Ulugbey ..	23 31 48	..	23 21 5	..	43

The whole of this observation established in an incontestable manner the successive diminution of the ecliptic. Their agreement with the formulas of *Méc. Cél.* leaves no room to doubt that this diminution is entirely owing to the

 attraction

traction of the planes over each other, and on the sun. The very small differences that still are found between the formulas and observations, being alternately negative and positive, show that no alteration is required in the value of the masses I have employed. These values are thus very near the truth; and to rectify them, we must wait for new observations, which can only be procured to astronomy by succeeding ages.

V. *Observations upon Luminous Animals.* By James Macartney, *Esq.**

The property which certain animals possess of emitting light is so curious and interesting, that it has attracted the attention of naturalists in all ages. It was particularly noticed by Aristotle and Pliny amongst the ancients, and the publications of the different learned Societies in Europe contain numerous memoirs upon the subject. Notwithstanding the degree of regard bestowed upon the history of luminous animals, it is still very imperfect; the power of producing light appears to have been attributed to several creatures which do not possess it; some species which enjoy it in an eminent degree have been imperfectly described, or entirely unobserved; the organs which afford the light in certain animals have not been examined by dissection; and lastly, the explanations that have been given of the phænomena of animal light are unsatisfactory, and in some instances palpably erroneous.

As this subject forms an interesting part of the history of organized beings, I have for some years availed myself of such opportunities as occurred for its investigation. Having communicated the result of some of my researches to the Right Hon. Sir Joseph Banks, he immediately offered me his assistance, with that liberality which so eminently distinguishes him as a real lover of science. I am indebted to him for an inspection of the valuable journal he kept during his voyage with Captain Cook; for permission to copy the original drawings in his possession, of those luminous animals discovered in both the voyages of Cook; and for some notes upon the luminous appearance of the sea, that were presented to him by Capt. Horsburg, whose accuracy of observation is already known to this learned Society.

In the following paper, I shall first examine the grounds on which the property of showing light has been ascribed to

* From the Philosophical Transactions for 1810, Part II.

to certain animals, that either do not possess it, or in which its existence is questionable. I shall next give an account of some luminous species, of which some have been inaccurately described, and others quite unknown. I shall endeavour to explain from my own observations, and the information communicated to me by others, many of the circumstances attending the luminous appearance of the sea. I shall then describe the organs employed for the production of light in certain species; and lastly, I shall review the opinions which have been entertained respecting the nature and origin of animal light, and relate the experiments I have made for the purpose of elucidating this part of the subject.

The property of emitting light has been reported to belong to several fishes, more particularly the mackarel, the moon-fish (tetraodon mola), the dorado, mullet, sprat, &c.

Mr. Bajon observed during the migration of the dorados, &c. that their bodies were covered with luminous points. These, however, proved upon examination to be minute spherical particles that adhered to the surface of these fishes; and, he adds, appeared to be precisely the same sort of points that illuminated the whole of the sea at the time. They were therefore in all probability the minute kind of medusa, which I shall have occasion to describe hereafter.

Godeheu de Riville states, in a paper sent to the Academy of Sciences at Paris, that on opening the scomber pelamis while alive, he found in different parts of its body an oil which gave out much light: but it should be observed, that Riville had a particular theory to support, for which this fact was very convenient, and that other parts of his memoir bear marks of his inaccuracy. It may be added, that if the oil of fishes were usually luminous, which Riville supposed, it would be almost universally known, instead of resting on a solitary observation.

As far as I am able to determine from what I have seen, the faculty of exhibiting light during life does not belong to the class of fishes. It appears probable, that some fishes may have acquired the character of being luminous, from evolving light soon after death.

Some species of lepas, murex, and chama, and some star-fish have been said to possess the power of shining; and the assertion has been repeated by one writer after another, but without quoting any authority.

Brugueire upon one occasion saw, as he supposed, common earthworms in a luminous state; all the hedges were filled

filled with them; he remarked that the light resided principally in the posterior part of the body*.

Flaugergues pretended to have seen earthworms luminous in three instances: it was at each time in October; the body shone at every part, but most brilliantly at the genital organs †.

Notwithstanding this concurrence of testimony, it is next to impossible, that animals so frequently before our eyes as the common earthworms should be endowed with so remarkable a property, without every person having observed it. If they only enjoyed it during the season for copulation, still it could not have escaped notice, as these creatures are usually found joined together in the most frequented paths, and in garden walks.

In different systems of natural history, the property of shining is attributed to the cancer pulex. The authorities for this opinion are Hablitzl, and Thules and Bernard. The former observed, upon one occasion, a cable that was drawn up from the sea exhibit light, which upon closer inspection was perceived to be covered by these insects ‡. Thules and Bernard reported that they met with a number of this species of cancer on the borders of a river, entirely luminous §. I am nevertheless disposed to question the luminous property of the cancer pulex, as I have often had the animal in my possession, and never perceived it emit any light.

The account given by Linnæus of the scolopendra phosphorea is so improbable and inconsistent, that one might be led to doubt this insect's existence, particularly as it does not appear to have been ever seen, except by Ekeberg, the captain of an East Indiaman, from whom Linnæus learnt its history.

I now proceed to the description of those luminous animals that have been discovered by the Right Honourable Sir Joseph Banks, Captain Horsburg, and myself.

On the passage from Madeira to Rio de Janeiro, the sea was observed by Sir Joseph Banks to be unusually luminous, flashing in many parts like lightning. He directed some of the water to be hauled up, in which he discovered two kinds of animals that occasioned the phænomenon; the one, a crustaceous insect which he called the cancer fulgens; the other, a large species of medusa, to which he gave the name of pellucens.

* *Journal d'Histoire Naturelle*, tome ii. † *Journal de Physique*, tome xvi.
‡ *Hablitzl ap. Pall. n. Nord. Beytr.* 4, p. 396.
§ *Journal de Physique*, tome xxviii.

The

The cancer fulgens bears some resemblance to the common shrimp; it is however considerably less, the legs are furnished with numerous setæ. The light of this animal, which is very brilliant, appears to issue from every part of the body.

The medusa pellucens measures about six inches across the crown or umbella; this part is marked by a number of opake lines, that pass off from the centre to the circumference. The edge of the umbella is divided into lobules, which succeed each other, one large and two small ones alternately. From within the margin of the umbella, there are suspended a number of long cord-shaped tentacula. The central part of the animal is opake, and furnished with four thick irregularly shaped processes, which hang down in the midst of the tentacula.

This zoophyte is the most splendid of the luminous inhabitants of the ocean. The flashes of light emitted during its contractions, are so vivid as to affect the sight of the spectator.

In the notes communicated to Sir Joseph Banks by Captain Horsburg, he remarks that the luminous state of the sea between the tropics is generally accompanied with the appearance of a great number of marine animals of various kinds upon the surface of the water; to many of which he does not, however, attribute the property of shining. At other times, when the water which gave out light was examined, it appeared only to contain small particles of a dusky straw colour, which dissolved with the slightest touch of the finger. He likewise observes, that in Bombay, during the hot weather of May and June, he has frequently seen the edges of the sea much illuminated by minute sparkling points.

At sunrise on April 12, 1798, in the Arabian sea, he perceived several luminous spots in the water, which conceiving to be animals, he went in the boat and caught one. It proved to be an insect somewhat resembling in appearance the wood-louse, and was about one-third of an inch in length. When viewed with the microscope, it seemed to be formed by sections of a thin crustaceous substance. During the time that any fluid remained in the animal, it shone brilliantly like the fire-fly.

In the month of June in the same year, he picked up another luminous insect on a sandy beach, which was also covered with a thin shell; but it was of a different shape, and a larger size than the animal taken in the Arabian sea.

By comparing the above description with an elegant pen-

and-

and-ink drawing which was made by Captain Horsburg, and accompanied his paper, I have no doubt that both these insects were monoculi; the first evidently belongs to the genus limulus of Muller; I shall therefore beg leave to distinguish it by the name of limulus noctilucus.

My pursuits, and the state of my health, having frequently led me to the coast, I have had many opportunities of making observations upon the animals which illuminate our own seas. Of these I have discovered three species: one of which is a beroe not hitherto described by authors; another agrees so nearly with the medusa hemispherica, that I conceive it to be the same, or at least a variety of that species; the third is a minute species of medusa, which I believe to be the luminous animal so frequently seen by navigators, although it has never been distinctly examined or described.

I first met with these animals in the month of October 1804, at Herne Bay, a small watering-place upon the northern coast of Kent. Having observed the sea to be extremely luminous for several nights, I had a considerable quantity of the water taken up. When perfectly at rest, no light was emitted, but on the slightest agitation of the vessel in which the water was contained, a brilliant scintillation was perceived, particularly towards the surface; and when the vessel was suddenly struck, a flash of light issued from the top of the water, in consequence of so many points shining at the same moment. When any of these sparkling points were removed from the water, they no longer yielded any light. They were so transparent, that in the air they appeared like globules of water. They were more minute than the head of the smallest pin. Upon the slightest touch, they broke and vanished from the sight. Having strained a quantity of the luminous water, a great number of these transparent corpuscles were obtained upon the cloth, and the water which had been strained did not afterwards exhibit the least light. I then put some sea water that had been rendered particularly clear, by repeated filtrations, into a large glass, and having floated in it a fine cloth, on which I had previously collected a number of luminous points, several of them were liberated, and became distinctly visible in their natural element, by placing the glass before a piece of dark-coloured paper. They were observed to have a tendency to come to the surface of the water, and after the glass was set by for some time, they were found congregated together, and when thus collected in a body, they had a dusky-straw colour, although individually

dually they were so transparent as to be perfectly invisible, except under particular circumstances. Their substance was indeed so extremely tender and delicate, that they did not become opake in distilled vinegar or alcohol, until immersed in these liquors for a considerable time.

On examining these minute globules with the microscope, I found that they were not quite perfect spheres, but had an irregular depression on one side, which was formed of an opake substance, that projected a little way inwards, producing such an appearance as would arise from tying the neck of a round bag, and turning it into the body.

The motions of these creatures in the water were slow and graceful, and not accompanied by any visible contraction of their bodies. After death they always subsided to the bottom of the vessel.

From the sparkling light afforded by this species, I shall distinguish it by the name of medusa scintillans.

The night following that on which I discovered the preceding animal, I caught the two other luminous species. One of these I shall call the beroe fulgens.

This most elegant creature is of a colour changing between purple, violet, and pale blue: the body is truncated before, and pointed behind; but the form is difficult to assign, as it is varied by partial contractions, at the animal's pleasure. I have represented the two extremes of form that I have seen this creature assume: the first is somewhat that of a cucumber, which, as being the one it takes when at rest, should perhaps be considered as its proper shape: the other resembles a pear, and is the figure it has in the most contracted state. The body is hollow, or forms internally an infundibular cavity, which has a wide opening before, and appears also to have a small aperture posteriorly, through which it discharges its excrement. The posterior two-thirds of the body are ornamented with eight longitudinal ciliated ribs, the processes of which are kept in such a rapid rotatory motion, while the animal is swimming, that they appear like the continual passage of a fluid along the ribs. The ciliated ribs have been described by Professor Mitchell, as arteries, in a luminous beroe, which I suspect was no other than the species I am now giving an account of.

When the beroe fulgens swam gently near the surface of the water, its whole body became occasionally illuminated in a slight degree; during its contractions, a stronger light issued from the ribs; and when a sudden shock was communicated

municated to the water, in which several of these animals were placed, a vivid flash was thrown out. If the body were broken, the fragments continued luminous for some seconds, and, being rubbed on the hand, left a light like that of phosphorus: this however, as well as every other mode of emitting light, ceased after the death of the animal.

The hemispherical species that I discovered, had a very faint purple colour. The largest that I found, measured about three quarters of an inch in diameter. The margin of the umbella was undivided, and surrounded internally by a row of pale brown spots, and numerous small twisted tentacula: four opake lines crossed in an arched manner from the circumference, towards the centre of the animal: an opake irregular shaped process hung down from the middle of the umbella: when this part was examined with a lens of high powers, I discovered that it was inclosed in a sheath in which it moved, and that the extremity of the process was divided into four tentacula, covered with little cups or suckers, like those on the tentacula of the cuttle-fish.

This species of medusa bears a striking resemblance to the figures of the medusa hemispherica, published by Gronovius and Muller; indeed it differs as little from these figures, as they do from each other. Its luminous property, however, was not observed by these naturalists; which is the more extraordinary, as Muller examined it at night, and says it is so transparent that it can only be seen with the light of a lamp. If it should be still considered as a distinct species, or as a variety of the hemispherica, I would propose to call it the medusa lucida.

In this species, the central part and the spot round the margin are commonly seen to shine on lifting the animal out of the water into the air, presenting the appearance of an illuminated wheel; and when it is exposed to the usual percussion of the water, the transparent parts of its body are alone luminous.

In the month of September 1805, I again visited Herne Bay, and frequently had opportunities of witnessing the luminous appearance of the sea. I caught many of the hemispherical and minute species of medusa, but not one of the beroe fulgens. I observed that these luminous animals always retreated from the surface of the water, as soon as the moon rose. I found also, that exposure to the daylight took away their property of shining, which was revived by placing them for some time in a dark situation.

In that season I had two opportunities of seeing an extended

tended illumination of the sea, produced by the above animals. The first night I saw this singular phænomenon was extremely dark; many of the medusa scintillans and medusa hemispherica had been observed at low water, but on the return of the tide they had suddenly disappeared. On looking towards the sea, I was astonished to perceive a flash of light of about six yards broad, extend from the shore, for apparently the distance of a mile and a half, along the surface of the water. The second time that I saw this sort of light proceed from the sea, it did not take the same form, but was diffused over the surface of the waves next the shore, and was so strong, that I could for the moment distinctly see my servant, who stood at a little distance from me; he also perceived it, and called out to me at the same instant. On both these occasions the flash was visible for about four or five seconds; and although I watched for it a considerable time, I did not see it repeated.

A diffused luminous appearance of the sea, in some respects different from what I have seen, has been described by several navigators.

Godeheu de Riville saw the sea assume the appearance of a plain of snow on the coast of Malabar*.

Captain Horsburg, in the notes he gave to Sir Joseph Banks, says, there is a peculiar phænomenon sometimes seen within a few degrees distance of the coast of Malabar, during the rainy monsoon, which he had an opportunity of observing. At midnight the weather was cloudy, and the sea was particularly dark, when suddenly it changed to a white flaming colour all around. This bore no resemblance to the sparkling or glowing appearance he had observed on other occasions in seas near the equator, but was a regular white colour, like milk, and did not continue more than ten minutes. A similar phænomenon, he says, is frequently seen in the Banda sea, and is very alarming to those who have never perceived or heard of such an appearance before.

This singular phænomenon appears to be explained by some observations communicated to me by Mr. Langstaff, a surgeon in the City, who formerly made several voyages. In going from New Holland to China, about half an hour after sunset, every person on board was astonished by a milky appearance of the sea: the ship seemed to be surrounded by ice covered with snow. Some of the company supposed they were in soundings, and that the coral bottom gave this curious reflection; but on sounding with 70 fa-

* *Mem. Etrang. de l'Acad. des Sc.* tome iii.

thoms

thoms of line no bottom was met with. A bucket of water being hauled up, Mr. Langstaff examined it in the dark, and discovered a great number of globular bodies, each about the size of a pin's head, linked together. The chains thus formed did not exceed three inches in length, and emitted a pale phosphoric light. By introducing his hand into the water, Mr. Langstaff raised upon it several chains of the luminous globules, which were separated by opening the fingers, but readily re-united on being brought again into contact, like globules of quicksilver. The globules, he says, were so transparent, that they could not be perceived when the hand was taken into the light.

This extraordinary appearance of the sea was visible for two nights. As soon as the moon exerted her influence, the sea changed to its natural dark colour, and exhibited distinct glittering points, as at other times. The phænomenon, he says, had never been witnessed before by any of the company on board, although some of the crew had been two or three times round the globe.

I consider this account of Mr. Langstaff very interesting and important, as it proves that the diffused light of the sea is produced by an assemblage of minute medusæ on the surface of the water.

In June 1806, I found the sea at Margate more richly stored with the small luminous medusæ than I have ever seen it. A bucket of the water being set by for some time, the animals sought the surface, and kept up a continual sparkling, which must have been occasioned by the motions of individuals, as the water was perfectly at rest. A small quantity of the luminous water was put into a glass jar, and on standing some time, the medusæ collected at the top of the jar, and formed a gelatinous mass, one inch and a half thick, and of a reddish or mud colour, leaving the water underneath perfectly clear.

In order to ascertain if these animals would materially alter their size, or assume the figure of any other known species of medusa, I kept them alive for 25 days, by carefully changing the water in which they were placed; during which time, although they appeared as vigorous as when first taken, their form was not in the slightest degree altered, and their size but little increased. By this experiment I was confirmed in the opinion of their being a distinct species, as the young actiniæ and medusæ exhibit the form of the parent in a much shorter period than the above.

In September 1806, I took at Sandgate a number of the beroe

beroe fulgens, but no other species: they were of various dimensions, from the full size down to that of the medusa scintillans: they could, however, be clearly distinguished from the latter species, by their figure.

Since that time, I have frequently met with the medusa scintillans on different parts of the coast of Sussex, at Tenby, and at Milford haven. I have likewise seen this species in the bays of Dublin and Carlingford in Ireland.

In the month of April, last year, I caught a number of the beroe fulgens in the sea at Hastings; they were of various sizes, from about the half of an inch in length to the bulk of the head of a large pin. I found many of them adhering together in the sea: some of the larger sort were covered with small ones, which fell off when the animals were handled; and, by a person unaccustomed to observe these creatures, would have been taken for a phosphoric substance. On putting a number of them into a glass containing clear sea water, they still showed a disposition to congregate upon the surface. I observed that when they adhered together, they showed no contractile motion in any part of their body, which explains the cause of the pale or white colour of the diffused light of the ocean. The flashes of light which I saw come from the sea at Herne Bay, were probably produced by a sudden and general effort of the medusæ to separate from each other, and descend in the water.

The medusa scintillans almost constantly exists in the different branches of Milford haven that are called pills. I have sometimes found these animals collected in such vast numbers in those situations, that they bore a considerable proportion to the volume of the water in which they were contained: thus, from a gallon of sea water in a luminous state, I have strained above a pint of these medusæ. I have found the sea under such circumstances to yield me more support in swimming, and the water to taste more disagreeably than usual; probably the difference of density, that has been remarked at different times in the water of the sea, may be referred to this cause.

All my own observations lead me to conclude, that the medusa scintillans is the most frequent source of the light of the sea around this country; and by comparing the accounts of others with each other, and with what I have myself seen, I am persuaded that it is so likewise in other parts of the world. Many observers appear to have mistaken this species for the nereis noctiluca, which was very natural, as they were prepossessed with the idea of the frequent existence of the one, and had no knowledge of the

other. Some navigators have actually described this species of medusa, without being aware of its nature. Mr. Bajon, during his voyage from France to Cayenne, collected many luminous points in the sea, which, he says, when examined by a lens, were found to be minute spheres. They disappeared in the air. Doctor Le Roy, in sailing from Naples to France, observed the sparkling appearance of the sea, which is usually produced by the medusa scintillans. By filtering the water, he separated luminous particles from it, which he preserved in spirits of wine: They were, he says, like the head of a pin, and did not at all resemble the nereis noctiluca, described by Vianelli; their colour approached a yellow brown, and their substance was extremely tender, and fragile. Notwithstanding this striking resemblance to the medusa scintillans, Le Roy, in consequence of a preconceived theory, did not suppose what he saw were animals, but particles of an oily or bituminous nature *.

The minute globules seen by Mr. Langstaff in the Indian ocean, were, I think, in all probability, the scintillating species of medusa; and on my showing him some of these animals I have preserved in spirits, he entertained the same opinion.

Professor Mitchell, of New York, found the luminous appearance on the coast of America to be occasioned by minute animals, that from his description plainly belonged to this species of medusa, notwithstanding which, he supposed them to be a number of the nereis noctiluca †.

The luminous animalcule discovered by Forster off the Cape of Good Hope, in his voyage round the world, bears so strong a resemblance to the medusa scintillans, that I am much disposed to believe them the same. He describes his animalcule as being a little gelatinous globule, less than the head of a pin; transparent, but a little brownish in its colour; and of so soft a texture that it was destroyed by the slightest touch. On being highly magnified, he perceived on one side a depression, in which there was a tube that passed into the body, and communicated with four or five intestinal sacs. The pencil drawings he made on the spot are in the possession of Sir Joseph Banks, by whose permission engravings from them are subjoined to this paper. By comparing these with the representations of the medusa scintillans, and some of this species rendered visi-

* *Observ. sur un Lumiere produite par l'Eau de la Mer. Mem. Etrang. des Sc.*
† Phil. Mag. vol. x. p. 20.

ble,

ble, by being a long time preserved in spirits, which I have laid before this learned Society, it will be found, that the only difference between Forster's animalcule, and the medusa scintillans, is in the appearance of the opake parts, shown in the microscopic views.

Many writers have ascribed the light of the sea to other causes than luminous animals. Martin supposed it to be occasioned by putrefaction: Silberschlag believed it to be phosphoric: professor J. Mayer conjectured that the surface of the sea imbibed light, which it afterwards discharged. Bajon and Gentil thought the light of the sea was electric, because it was excited by friction. Forster conceived that it was sometimes electric, sometimes caused from putrefaction, and at others by the presence of living animals. Fougeroux de Bondaroy believed that it came sometimes from electric fires, but more frequently from the putrefaction of marine animals and plants.

I shall not trespass on the time of the Society to refute the above speculations: their authors have left them unsupported by either arguments or experiments, and they are inconsistent with all ascertained facts upon the subject.

[To be continued.]

VI. *On Dr.* A. Walker's *Opinion, respecting the general Deluge, the Formation of Mountains, the Ruptures of the Strata, &c. by the Approach of a Comet to the Earth.* *Communicated by Mr.* John Farey.

To Mr. Tilloch.

Sir, Happening lately to have met with a small work in 32mo, entitled, " An Account of the *Eidouranion*, or Transparent Orrery, invented by A. Walker, M.D.S. as lectured upon by his son W. Walker," the sixth edition, Bury, 1786, to which there is added, A Dissertation on the Deluge and other subjects connected with geology; I find therein some views of the subject, which I never remember to have elsewhere seen in print; and conceiving it probable that it may have been so with others of your geological readers, I request that you will give a place to this dissertation in your next number. I have no remarks at present to make on it, but am

Your obedient servant,

Westminster, Jan. 2, 1811. John Farey, Sen.

" *A Dissertation on the probable Cause of the Deluge.*

" So perfect are the laws by which this wonderful system

 is

is regulated, and so effectual that self-physic which the Almighty has instituted through all his works, that if any fortuitous accident happens in the system, there requires no immediate interposition to prevent or cure the mischief, each body carrying within itself the principles of preservation and cure; an argument of wisdom and foresight worthy of the Deity!

"The planet Jupiter was attracted out of his orbit by the enormous comet which appeared in the year 1680. The comet came across the plane of his track, had a temporary influence upon him, and it is observable, he has not travelled by the same fixed stars since that period which he did before it. When the influence of the comet had ceased, and he was again left to that of the sun as before, no doubt but his usual motion was momentarily retarded, and the shape of his orbit altered. Now if Jupiter consists of land and water (and by the spots seen on his face it is more than probable), it is possible he might experience a revolution something similar to our flood; for that *our flood was occasioned by the near approach of a comet*, is a most natural supposition, and in no wise militates against the Scriptural doctrine of that event: it being as easy, and as consistent for the Almighty, to render justice by a secondary cause, as by an immediate interposition. Nor is his attribute of mercy arraigned by the promiscuous destruction the deluge occasioned; for it is evident, by reasoning from his works, that he governs the universe by "general, not by partial laws."

"The vestiges of the deluge are so remarkable, both on the surface and within the bowels of the earth, that, if examined without prejudice, they prove, I think, beyond a doubt, that awful revolution to have been the work of a comet. Not that the moisture of its tail drowned the world, as was unphilosophically suggested by Whiston; but if the attraction of the moon be capable of raising the water of the sea above its common level, what effects might not be supposed from the nearer approach of a body perhaps many thousand times as large as the moon? If *a tide by such an attraction was raised three or four miles above the level of the sea*, the earth, by turning on its axis, would have that protuberance dragged over the land, and *its surface would be ploughed up into those inequalities we call mountains;* for that mountains are not of eternal duration, is evident from their growing less, even in the memory of man; for every thing strives at a level. Rains falling on mountains wash down their asperities; this matter bemuds the

the rivers, and banks out the sea: rocks themselves yield up their fantastic forms to the effects of air, water, and heat; and land has been growing into the water ever since the deluge. But why should all assemblages of mountains be arranged like the little ridges of sand on the sea shore? Doubtless by having been produced by a superior tide, and left to dry by an unreturning sea. Almost all great ranges of mountains run north and south: the Andes of the Cordeleras; the Mountains of the Moon in Africa; the Dofranes, Caucasus, Appenine, Allegany, &c.—the Alps and Pyrenees excepted.

"As comets visit our system in all directions, why might not that in question have its motion from north to south, and, dragging the sea after it, determine the mountains to those points of the compass? From whence come the shells and fish-bones we meet with on the tops of the highest mountains? We have not discovered any power in nature disposed to work such quantities of them through the bowels of the earth; and superstition has not yet been so mad as to carry them thither: they are not a fortuitous assemblage of atoms assuming such forms; not *lusus naturæ*, but *bonâ fide* shells and fish-bones, such as we meet with on the sea-shore! We find them also deep buried in the bowels of the ground, far from the sea; we find them in rocks, and often converted into stone; nay, why may not the fat of fish, joined with vegetable substances, form the bitumen of coal? We have experiments that warrant such a suggestion. Now, *if ever the sea was dragged over the surface of the earth by the attraction of a comet*, these effects must naturally follow.

"In digging into the bowels of the earth, we have still stronger evidence that the flood was occasioned by the near approach of a comet. It is well ascertained, that the united attraction of every atom of the earth forms that earth into a dense ball, and not any particular attraction in its centre. All matter being therefore affected by this power in proportion to its density, one might conclude that the heaviest bodies would lie deepest, and the lightest near the surface; but this is by no means the case: coal is lighter than stone; various minerals lie upon light earths, &c. evidently proving, that the general order of nature has at some time been disturbed, and the manner in which matter obeys the laws of gravity disarranged. Hence the philosophic miner finds strata of various density in digging downwards; and in pursuing his vein of ore, finds these strata broken and divided; nay, if he loses the vein, he can easily tell where to

find it again, by the manner in which it broke off. In this he is never mistaken; he sees it, as it were, through many fathoms of earth! Evidently suggesting, that some revolution on the earth has *broken up its naturally arranged strata*, and introduced this "regular confusion."

"The various strata of the earth seldom lie on one another horizontally: they generally dip; and near the shore commonly *incline towards the sea*. On the south coast of England, the rocks incline southerly; on the opposite coast of France, they incline to the north. Is it not probable, that at the deluge, the horizontal stratum was broken between these countries: and the ends falling lowest at the breach, formed the channel, into which the sea flowed, *when it lost the influence of the comet, and again obeyed the power of gravity?* Countries separated by narrow channels, universally have their shores inclining towards the sea; showing that the general geography was at that time altered.

"It is true, we have an old doctrine revived, and supported by respectable authority, that mountains were formed originally by these eruptions we call volcanoes. The votaries of this theory pronounce the hollows and cavities on the tops and sides of mountains, craters, or the cups of extinguished volcanoes; and if the stone of the mountain be of a blueish colour, then it is declared lava; and the proof of a volcano having existed there becomes incontrovertible! History, however, affords us very few instances of mountains so formed. This doctrine has received very just authority from the late scientific circumnavigators. The rocks which surround the islands of the Pacific Ocean, generally break off perpendicularly about a mile out at sea, which makes their approach very difficult and dangerous; and as the stratum immediately under the loam of the surface has an ashy or lava-like appearance, the voyagers very naturally concluded, that the immense number of small islands which stud that extensive ocean, were the product of subaqueous eruptions.

"If I might be allowed to hazard an opinion against such respectable authority, I should rather apprehend that the Pacific Ocean had been once a continent, and that at the deluge, *when the earth's surface was disarranged and broken up by the violent motion of the waters*, the general body of it sunk beneath the level, or was washed away to other parts, leaving only the more elevated and solid part remaining. For volcanoes throw up matter piecemeal; islands, therefore, formed by them would have a sloping, or gradually sinking shore; whereas the islands of the Great South Sea are

are *surrounded by perpendicular rocks*, that sink in that direction to an almost unfathomable depth in the sea. Besides, how can we account for that similarity of manners, customs, colour, and even language, among the inhabitants of islands so distant, that no mode of navigation they practise could ever make them acquainted, or to have any communication with one another? If these islands were thrown up from the bottom of the sea, their inhabitants would not be thrown up along with them, and all with the same customs and language. Now if this immense part of the globe was *a continent before the deluge*, the inhabitants might be alike; and if the elevated parts were above the waters, (a circumstance more than probable,) inhabitants might be saved upon them, with every circumstance of similarity we now find among them; for that revolution is not of so remote a date, but remains of antediluvian manners might exist at this time."

VII. *Reflections on some Mineralogical Systems.* *By* R. Chenevix, *Esq. F.R.S. and M.R.I.A.*, *&c. Translated entire from the French, with Notes by the Translator.*

[Continued from vol. xxxvi. p. 361.]

GERMAN SYSTEM OF NOMENCLATURE.

It would become fastidious and almost useless to dwell longer on the external characters of Werner. We can conceive the details into which this philosopher has entered; but to convey an idea of the minutiæ with which the Germans think it necessary to treat a subject, I shall give the shortest extract possible from 28 pages, in which Emmerling speaks of nomenclature. I intend this specimen as a historical and characteristic fragment; but I anticipate that the reader will dread the tediousness of following me, although I propose to favour him with a great part of the original.

I. Object of the nomenclature (*zweck der fossilien benennungen*) —It is necessary in every art or science to have words or names to designate the things of which we treat. They serve particularly to fix the different concrete ideas, to express ourselves intelligibly, and to communicate our thoughts.

II. Different sorts of names (*verschiedene arten derselben*).—Of these there are two; trivial, and systematic.

A. (*Triviallen benennungen*). The trivial names are those

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which minerals receive in common life, and often from miners. Every language possesses them, and some of them have even passed to others; as quartz, blende, &c.

The trivial names may be divided into two: general and particular trivial names.

1. The general trivial names (*algemeine triviallen benennungen*) are those commonly used in any language and received into another. They are important, inasmuch as they are used by philosophers themselves, who have divided them into two.

a. General trivial names (*haupt trivialnamen*) are those most generally known and used.

b. Accidental general trivial names (*neben trivialnamen*) are those least known; such as cat-gold or cat-silver (*argent de chat*) [lamelliforme mica, Haüy], Muscovy glass, for mica, &c.

2. The particular trivial names comprehend those used by certain classes of persons, and in certain countries. They are divided into three (*besondre trivialnamen*).

a. Provincial or local names (*provenziellen und local benennungen*). The peasantry in certain provinces use these names; *mispickel*, in Saxony, for arsenical pyrites; *gelf*, *gelferz* or *gelft*, is the Hungarian name of coppery pyrites.

b. Officinal names (*officinelle benennungen*) are those used by apothecaries and other traders in the shops; as bloodstone, nephretic, &c.

c. Technical names (*technologische benennungen*) are those used by artists or workmen. Statuaries call all kinds of stone which are easily wrought, marble.

B. Systematic names (*systematische benennungen*) are those used by philosophers; they should be characterized by precision and perspicuity.

III. Principles and rules for forming these names.

A. Rule for the formation of general trivial names.—Werner gives eight rules for this purpose. The names ought to be distinct, definite or clear (*unterscheidend*); just, with respect to the thing (*sachrichtig*); correct, with respect to language (*sprachrichtig*); descriptive (*bezeichnend*); short (*kurz*); fixed and applied only to a single mineral (*festgesetz*); unique, (*einzig*); and distinct from one another (*ausgeziechnet*).

1. A name is distinct when it belongs only to a single species: spar, schiste, and schorl, are not distinct; but feldspar, fluor spar, argillaceous schiste, &c., are.

2. It is just with respect to the thing, when it does not give

give a false idea of the mineral. Smoked topaz [*quartz hyalin enfumé* of Brogniart] transgresses in this point of view, since it is not a topaz.

3. Correct or accurate with respect to language, when, on being written or spoken, it is not inimical to the genius of the language. In this rule there are eight subdivisions.

a. When the name of a species is composed of a substantive and an adjective, the latter should be indeclinable. (Examples are cited in the German.)

b. When a name is composed of two words, that which gives the general idea should be first, and that which limits this idea, the last. (Examples of this rule are also given in German.)

c. When a name is composed of two, three, or four words, we should separate and unite them as the nature of the thing may require. Thus, *grau-spies-glaserz*, a mine of gray vitreous antimony.

d. All the names of species, whether simple or compound, should be written with an initial capital latter, as *Grau-spies-glaserz*. The varieties ought not to commence with a capital letter, but when their name differs from that of the species, as Amethyst, Prase. If there be an adjective to designate the variety, it ought to commence with a small letter; thus, *dichtes-Grau-spies-glaserz*.

e. The names ought to be taken from one language only, as chrysolithe.

f. We ought not to translate names taken from other languages*. We should not say gold-stone instead of chrysolithe.

g. We should write and pronounce the words according to their true signification and etymology. The name of a species is *rothgultigerz*; it is improper to say, *rothgulden*, *rothguldenerz*, *rothguldigeserz*. (When a man is named *Pierre* (Peter), he ought not to be called James, nor even *Pierrot*).

h. In forming new names, it is necessary to consult analogy. Thus, sedative spar, phosphoric spar, are bad names, because the name of spar is properly applied to earthy substances, and not to salts. (It has here been forgotten that

* This rule would be very proper if all the new names of minerals were derived from the Greek, as has been done by Haüy; but if the Germans suppose that the French, Italians, Spanish and English should adopt their barbarous combinations of letters to designate minerals, they only betray their vanity at the expense of their common sense. This was too much even for Mr. Jameson; and his fate will perhaps be a warning to others, never again to attempt to sacrifice both classical and vernacular language on the altar of German gothicism.—Trans.

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the word is employed in the formation of the names calk-spar, schiefferspar, braunspar, bitterspar, flutspar, schever-spar, wulfelspar, among the acidiferous substances; and only in the names demantspar, feldspar, and skhillespar, among the earthy minerals.)

4. Descriptive, in order to give an idea of the principal properties of minerals. There are six sources whence we should principally derive them, and four others which offer less advantage.

a. Some very remarkable external character. The word heavy spar, from its weight; olivin, from its colour; stink-stone [fetid carbonated lime of Haüy], from its odour, &c.

b. Some physical or chemical property: as zeolite, in consequence of its effervescence by the action of fire; mine of magnetic iron, &c.

c. Some constituent principle of the mineral: but the uncertainty of analyses militates a little against this source.

d. The usage of a mineral: as fire-stone [common flint, pyromacous silex, Linn. and Haüy], fullers' earth, porcelain earth, &c.

f. Resemblance to certain other objects in the usage of common life: as pitch-stone, horn-stone, &c.

The sources less proper for deriving names are comprehended in the history of the mineral.

a. Geographical situation: as calcedony, Labrador stone, &c.

b. Nature of the soil: as mine of marshes, mine of meadows (*monasterz*, *wiesenerz*).

c. The names of persons; but only when a philosopher has been the first to make known a mineral, its properties and uses: as Prehnite, Witherite, &c*.

d. Some trait in the history of a mineral: as appatite.

5. Short, as long names are difficult to pronounce and inconvenient to write and to remember. Two words at most should be employed. (The Germans have forgotten this rule in their language and in their names. *Dichtes-grau-speis-glas-erz*, is composed of five words.) *Dense-*

* With this proviso it seems difficult to determine who may claim the right to the honour of transferring his name to a mineral, and also whether his Christian or family name or title should be preferred. Should the discoverer not happen to be an author or professor, it appears that he has no right to the honour; and if he carry the mineral to Werner or any of his disciples, then it may lawfully be baptized with the name of the professor of Freyberg, or any of his followers who are called *learned*. The folly and vulgarity of such a system of nomenclature must be sufficiently evident; and were any proof wanting of its total inadequacy, the very example cited will furnish it, as every English reader must naturally ascribe the name Witherite rather to Dr. Withers than Withering.—Trans.

gris-

gris-antimoine-verre-mine, or, in French, *mine d'antimoine grise vitreuse en masse,* mine of gray vitreous antimony in a mass.

6. Fixed and applied to only one mineral. Plombagine has designated carburetted iron and sulphuretted molybdena.

7. Unique or singular; each mineral should have only one name: but here there is a real chaos in mineralogy.

8. Distinct from all others, in order to avoid confusion.

The question now is to know in what case we should introduce a new name.

1. When a new mineral is discovered.

2. When chemical analyses change the place of a mineral.

3. When a name transgresses the rules of a language or of analogy.

4. When a name is in contradiction with the known properties of a mineral.

5. When a name is applied to several minerals.

B. Rules for forming systematic names.

1. They should be taken from the learned languages.

2. They ought to be taken from among the principal trivial names, and at the same time to mark the genus to which the fossil belongs.

3. The name of the genus must be placed first, that of the species should follow; as *silex quartzum, silex quartzum amethystus.* The trivial name may be placed in a parenthesis.

4. Names taken from the Greek and Latin, like other trivial names, should be retained and employed with a Latin termination: as zeolithus, serpentinus, creta.

5. Finally, in the formation of systematic names, we must observe the rules already given for the formation of trivial names.

Such a mode of teaching might suit the borders of the Ohio, but cannot now be admitted in Europe.

I have attended in Germany fourteen lectures in the form of prolegomena, and a course of mineralogy, of which the following is the quintessence: "Gentlemen, it was necessary for the study of mineralogy that the world was created, and that man was made."

In the same course it was affirmed that the town in which it was delivered was built in consequence of the mines which are found in its vicinity, and on no account that the mines were discovered in consequence of the contiguity of the town.

If we give a block of marble to Praxiteles, and another to a common man, Praxiteles would make of his an Apollo, a Venus,

a Venus, a Laocoon: under the chissel of another, nothing would result but sparks and powder.

One should expect that principles laid down so much at length might be rigorously followed. I have cursorily remarked some deviations: the following are some others.

In geognosis there is a rock called *weisstein*, white-stone. I have heard, in a public course, the description of this rock in these term: *Das weisstein ist grau*, "the *white*-stone is *gray*."

Such names as cubicite, octaedrite, ought not to be adopted but when a mineral is the only one which possesses the forms indicated by these words, or which possesses no others.

But, what is still worse, is to introduce contradictions and inaccuracies of this kind into a part of mineralogy, the determination of which belongs to more profound knowledge and more elevated faculties of the human mind; in that part, the merit of which, if it be not entirely owing to accuracy, at least cannot exist without it: I mean, that it is a much more serious fault to apply mathematical, and consequently rigorous, names, to designate a thing which is not what the name inplies. *Wurfel zeolithe* is an example of this: the word means, cubic zeolite*. Under this denomination two of Haüy's species (analcime and chabasie) are comprehended (Brochant, vol. i. p. 304). Analcime, indeed, is cubic; but chabasie is a rhomboid, which differs from a cube 3°,30. The most vulgar empiricism could not excuse such a fault. The joiner or mason who had not a more correct idea of a cube, would deserve to remain unemployed.

It was at Freyberg that, for the first time in my life, I heard of a square with oblique sides (*geschobene quadrat*); of an *almost* cube (*fast wurfel*), &c.

A word which agrees perfectly with these principles, but which is itself extremely vicious, is *oryctometry*. It is said that the system of Haüy is properly oryctometry, or the art of measuring fossils. I maintain that this idea could not be suggested but by ignorance which misconceived the principles of the author, or by disingenuousness which sought to pervert them. Were nothing more to be done than measuring fossils, it would require neither the talents of a man of learning, the calculation of a geometer, the reasoning of

* On this mineral Mr. Jameson observes, "Haüy has formed of this subspecies two distinct species, but *without sufficient reason*." Yet he has not shown why the crystallographer's reason is insufficient, if he really knew it; nor even condescended to communicate any collateral proof, or miscellaneous knowledge, tending to support the practice of his master.—Trans.

a lo-

a logician, nor the extended views of a philosopher. The first surveyor, the first joiner, might be as able as M. Haüy. Doubtless, it is necessary to measure certain relations of the figures of crystals, and estimate the angles and the sides; but not to banish these results into a note, as a celebrated German professor desires, and contents himself with saying, that carbonated lime presents the figure of a rhomboid whose angles have such or such a measure. It is to form the text, the base of a work, to extract from these results superior consequences which influence the whole mineral kingdom, and distribute it in a natural and luminous order; it is to set out from the point where empiricism ceases to see, and to raise one's self to conclusions which the philosopher alone can seize. Oryctometry is the manual part of Haüy's system, that which the eyes perceive; the remainder is for the understanding. No one has ever said that astronomy is astrometry or uranometry, although the heavens and the stars are measured. The chemist boils his acids, roasts his minerals, and blows the bellows of his forge; the calculator covers the leaves of his paper with rough signs and letters without order; the poet cuts his pen and dips it into ink: this is what all eyes may observe. But woe to the folly which limits the sallies of genius which produced the Iliad, or conceived the doctrine of infinitesimals, to these manipulations.

I have heard M. Haüy reproached for misemploying the name phosphated lime, in giving it to that which, in the system of Werner, forms two different species, appatite and asparagus-stone. The fault is his who made two species of the same mineral.

M. Haüy has effected a great reform in the nomenclature of minerals. He is justly of opinion, that significant names which recall some characteristic property of the mineral to be named, or some circumstance relative to its history, are the most advantageous. Others prefer insignificant names for simple substances, and require significant names for compounds, in order to give an idea of their nature. Thus, in chemistry, they would banish the words oxygen and hydrogen; but sulphuric acid and sulphat of lime, appear to them excellent. The species are simple bodies, the unities of mineralogy, and consequently should have insignificant names *. M. Haüy has created nearly the half of the names which

* It is difficult to conceive any possible advantage which could accrue to science from arbitrary names; they were never imposed on any thing in nature, but from ignorance and necessity. It may, indeed, be questioned, whether any human being ever designated any one thing by an appellation which

which he uses in his system. He has been reproached with having derived them from the Greek, because to comprehend them it is necessary to have studied that language. But to those who know not Greek, these names will enter into the class of insignificant; that is to say, among those which many persons consider as the best.

RESULT OF THE SENSUAL SYSTEM OF CHARACTERS—DIFFERENCE BETWEEN EMPIRICISM AND SCIENCE.

If we consider the mineralogical system of Werner with respect to these two questions,—Whence did it set out? where is it gone?—the answers will be very different. It is true, that the state of mineralogy was very deplorable before him; and all that he found in ancient authors was so vague and ill-conceived, founded on ideas so erroneous, that it was more likely to lead him into errors than to guide him. He has indeed followed the same path as they; but we may say, that if they have travelled it before him, if they have indicated it to him, they have furnished him with very few of the means of rendering it practicable; and he has all the merit of setting out from a very remote point, to take new steps in aid of his own labour.

But has he arrived as far as he might have done, had he profited by every thing which surrounded him? This is what I cannot absolutely concede to him. Besides the objections

which had no analogy, no real or imaginary relation, to something previously known, and that he studied to find an articulate sound for which he felt no predilection, no choice, nor reason. When we consider the universal and ever active principle of the association of ideas, we may venture to affirm that man cannot form any new term which is wholly and absolutely insignificant; but that it must have some latent analogy, some similarity or affinity either with personal feeling, caprice, or conceit. The question then is, whether it is more philosophical and scientific to adopt names called insignificant, but which are really founded on some fancy or caprice, as the ancients transferred the names of animals to certain parcels of stars; or appellations derived from some efficient inherent principle, and founded on the ablest efforts of reason at the period of their adoption. The former have flattered the vanity and excited the enthusiasm of weak minds, but never smoothed the way to any discovery or improvement; whereas the latter always awaken new associations in the mind which receives them, expand the actual basis of knowledge, and become the stepping-stones to new, and, if possible, still more important discoveries. The natural and literary history of the terms oxygen, hydrogen, oxymuriatic and muriatic acids, demonstrates the truth of this observation; and had these substances been designated by arbitrary and insignificant names, or the names of their accidental discoverers, it is more than probable that half the experiments to which they have been subjected, in order to ascertain the propriety or impropriety of their nomenclature, would never have been performed, nor the science of chemistry be so far advanced in mathematical certainty and practical utility as we now find it. Werner's practice of mineralogical nomenclature is unquestionably the most pernicious part of his dogmatic system.—TRANS.

jections in the details which I have already made to two parts of his system, I shall make three other charges generally to the method of Werner. He has made all his appeals to very incompetent judges—our senses; he has stopped to describe instead of defining; and he has attempted to exclude the aid of other sciences in order to render mineralogy independent in its means.

Far from avowing that the first of these objections is really one, the partizans of Werner consider it as a very great advantage that he appeals solely to our senses, as they are the unchangeable judges of properties which do not vary; and much stress is laid on the stability of external characters. While the other means of diagnosis and of classification change, it is said, from day to day, the external characters remain the same; and that which has served to distinguish a mineral to-day, will serve the same during a thousand years; whereas, on the contrary, not a month passes without some considerable change in chemistry*. The senses of the human species, according to all appearance, are the same that they were since the creation of man; and the properties of the mineral kingdom have no more changed than they. But from one individual to another their perfection and their delicacy constantly vary. This stability, therefore, of the means of judging external characters, is but apparent; it exists from one generation to another, but it is null between individuals. Hence the stability of external characters becomes illusory.

But, even granting to the applications of our senses more advantages than they really posses, their effect would be to retard the progress of all knowledge in which they should be employed as exclusive means, and to prevent us from ever attaining that state which is designated by the name of science. It must be remembered, that the march of the latter is progressive; that each day adds something to its precision, and some principle which becomes the basis of new improvements. Its treasures are like an inheritance which prospers in the hands of those to whom it has devolved; they offer incessantly new riches, added to those which have already been amassed during preceding centuries.

* This capability of change occurs only in those arts and sciences which are susceptible of improvement with the progress of knowledge. To boast therefore of the uniform stability or stationariness of any science, is but to convert one of the greatest defects in any branch of human knowledge into a superlative merit. If mineralogy was henceforward to remain in the state it is now placed by Werner, it would be as useless to society, as unworthy of human study and genius, as the fancied music of the spheres was to the ancients.—Trans.

turies. But the faculty of appreciating qualities immediately sensible, not requiring either profound study or complicated reasoning, sooner attains its highest point, since the improvement of our senses by the habit of using them is the most certain way to succeed. No series of words can represent the *tact* which distinguishes a practitioner from a theorist; no precept can communicate it. He who has passed a long life in seeing, touching, and feeling, even when he has done every thing for himself, has no legacy to leave to posterity. In the balance, which I suppose at first to have been a simple pole suspended by the middle, imperfections were seen and attempted to be corrected; possible meliorations were perceived and executed. The instruments themselves have been the registers of these changes, and we have many of them less imperfect than those of our ancestors. Yet he who could have acquired the faculty of judging of masses near to the millionth part, would have contributed less to the instruction of after ages than the balance which would have been broken.

If the external characters are so stable as Werner tells us; and they are so in effect; why has he so often changed the place of all minerals one after another in his system? Is it that the colour, the specific gravity, the hardness, are not the same this year as the last? Have they not the same value; or are they not seen with the same eye? If by the extension of means, by the acquisition of new knowledge, he had prepared rational changes; if he had substituted a verity in place of an error, his system would have truly gained. But simply to change one truth for another, or put one error in the place of another, is not a progressive march in the sciences.

As to the second of these objections, it appears sufficiently proved by all which bears the name of accurate knowledge, that definitions are the language of the sciences. The mathematics rest on definitions; chemistry and physics can define much; natural history, rarely. Zoology and botany may define the generalities and describe the details. Mineralogy describes both, and defines almost nothing. Not that a descriptive method is unnatural; it is without doubt the first which men used. If we open our eyes on the grand features of Nature, the language of description is all that will remain with us, as it belongs to astonishment and admiration. It lends itself to all our sentiments, it alters not the vivacity of them, and we may almost add that to exaggerate with it is not to pervert truth. It especially suits that majestic harshness of nature which refuses our rules

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and revolts against our sciences: but whatever may be its charms, it has no principles but our sensations; and a science purely descriptive is a contradiction.

Among thousands of persons who consider the same thing, each will describe it according to his own ideas, and no one will be able to recognise it by the picture which another shall have made. But a definition is the same to all men; it is precise. If it totter, it is overthrown; whereas descriptions are satisfied with coming near. If by the one we are less exposed to commit errors, because we have established principles; by the other we are less in a condition to prove that we have been deceived, as we have followed no principle.

This is not the only inconvenience of a descriptive method. He who wishes to give a description, examines the object entire and in detail. He varies the aspect in a thousand different manners, till he believes that he has exhausted its sensible properties. If the object be one of those which are susceptible of classification, and if it is a species which we wish to describe, this species suffers itself to be divided into varieties, and these varieties are composed of individuals whose number is almost infinite. It is therefore necessary to comprehend in the description all the essential and accidental qualities of the known individuals; otherwise, it would be right to refuse a place in the species to all those which had not this quality, or which could have it from others. If the description be well done,—if it be given in detail,—we shall have a picture which will present the whole of all the scattered features in divers individuals: but if there be some property wanting in any of these individuals, this property excludes the species; for the species can have neither more nor less than what is found in the individual. To find the true specific character then, we must survey all the particulars of this picture, to discard what belongs not in common to all the individuals, for the rest is useless as a means of specification. We must search, in order not to see; and learn, expressly to forget.

Let us illustrate this by example.—In the work of Brochant we find that the first subspecies of fluor has two colours, the second three, and the third four. What then is the colour of the species? If all belong to it, we might represent fluor by the palette of the painter. If it has only one, why not name it without mentioning others? If none properly belongs to it, why cite the colour as a specific character?

If we view mineralogy only with respect to its application to the useful arts, and particularly to that of the miner, we might limit our knowledge to the simple diagnosis of minerals, and the most prompt and easy means would be the best; but if we conceive it capable of being placed in the highest rank, we shall never attain this object but by the assistance of knowledge derived from analogous sciences. It is by having long confounded specification with diagnosis, the philosopher with the miner, that we have not distinctly followed the different objects that have been proposed, and that we have not estimated the means employed according to their just value. It must indeed be confessed that the superiority of Haüy's system over that of Werner is infinitely greater in that part which relates to the establishment of species, or the philosophy of the science, than in the art of knowing minerals. We cannot expect that the miner, in exploring the earth, should select the specimens which he meets, distinguish them by their geometric figures, or seek every direction of the cleavage for the integral molecule. He must have more simple and more expeditious means: without a great effort of mind he must be able to avoid error; and provided that he does not greatly deceive himself respecting the contents of the metal which he explores, he is allowed to be not very rigorous as to the species of the individual. The jeweller may make as many species as colours; the weak virtuoso may dispute on the prism of emerald, jasper, oriental and occidental stones, &c.; but the naturalist should decide rather with certainty than celerity: he neither wishes to deceive himself nor diffuse a false light on science. He can return to the same object every time he pleases, can revise or correct his ideas, remould his opinions, diversify their sources, compare them, discuss them, and leave his doubts an inheritance to science; for his discoveries, and the moments which he has employed in hesitation, are precious to truth; while that time, with the artist, is an element of expense, and promptitude indemnifies him for less perfection. With justice might the latter blame the length and difficulty of the researches necessary to acquire the indispensable certainty that a mineral belongs to such or such a species, according to the principles of Haüy. But when I have heard this objection made by a man of learning, who professes mineralogy as a science, I have not been able to conceal my astonishment. "How would you do, (said he to me,) when one must pronounce on some hundreds of minerals in an hour or two?" To the botanist,

botanist, all the time necessary to his inquiries is allowed: the zoologist may consult anatomy and physiology at his leisure; he can observe the habits of animals: the gardener must not wait the time of inflorescence to distinguish plants: the butcher should know an ox or an ewe without being obliged to ascertain if they could reproduce beings fecund and similar to themselves: in like manner the miner ought not to stop at particulars: but I do not see why, among all those who study nature, the mineralogist should be the only one condemned to live in a perpetual hurry.

[To be continued.]

VIII. *On Cystic Oxide, a new Species of Urinary Calculus.* *By* William Hyde Wollaston, *M.D. Sec.R.S.**

The principal design of the present essay is to make known the existence, and to describe the leading properties, of a new species of urinary calculus from the human bladder; but I shall at the same time take the opportunity of correcting an inaccuracy or two that I have observed in my former communication on this subject. (Phil. Trans. 1797.)

I on that occasion took notice of five kinds of urinary calculi,

1. The *lithic acid*, since called *uric acid*, originally analysed by Scheele.
2. The *oxalate of lime*, or *mulberry calculus*.
3. The phosphate of lime, or *bone-earth calculus*.
4. The ammoniacal phosphate of magnesia.
5. The *fusible calculus*, which consists of the two last species combined.

It is now about five years since I first met with another species, evidently differing from each of those before described. It was in the possession of Dr. Reeve of Norwich, who obligingly gave me a portion of it for the purpose of examining its chemical qualities. It had been taken from his brother when he was five years old, and at that time was covered with a coating of phosphate of lime very loose in its texture, and consequently very soon separated†. This species is probably very rare; for, although I have omitted

* From Philosophical Transactions for 1810, Part II.

† I am informed, that another stone formed afterwards in the bladder of this boy, and that he died in consequence, without submitting to the operation a second time. The stone found in his bladder after death, consisted principally of uric acid, but was peculiar in one respect, as its centre was hollow by the removal of some more soluble substance, of which the nucleus had consisted.

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no opportunity of paying attention to any urinary concretions to which I could have access, I have, to this time, seen only one other specimen of the same substance. This last is in a collection of calculi belonging to Guy's hospital, given by Mr. Lucas, surgeon to that institution, having been formed partly by his father, and partly by himself, in the course of their practice; and according to the present arrangement, (which, it is to be hoped, will not be altered) the calculus to which I allude may be found by reference to No. 46 of that collection. It was extracted by the usual operation, from a man of 36 years of age, of whom no record is preserved, except that his name was William Small. It weighed, when entire, 270 grains.

In appearance, these calculi resemble more nearly the triple phosphate of magnesia, than any other calculus; but they are more compact than that compound is usually found to be: not consisting of distinct laminæ, but appearing as one mass confusedly crystallized throughout its substance. Hence, instead of having the opacity and whiteness observable in fusible calculi, which consist of a number of small crystals cemented together, these calculi have a yellowish semi-transparency; and they have also a peculiar glistening lustre, like that of a body having a high refractive density.

When this substance is submitted to destructive distillation, it yields fœtid carbonate of ammonia, partly fluid, and partly in a solid state, and a heavy fœtid oil, such as usually proceeds from animal substances; and there remains a black spongy coal, much smaller in proportion than is found after the distillation of uric calculi.

Under the blow-pipe it may be distinguished from uric acid by the smell, which at no period resembles that of prussic acid; but in addition to the usual smell of burnt animal substances, there is a peculiar fœtor, of which I cannot give a correct idea, as I know no smell which it can be said to resemble.

This species of calculus is so readily acted upon by the generality of common chemical agents, that its character may perhaps be most distinctly marked, by an enumeration of those feeble powers that it can resist.

It is not dissolved (excepting in very small proportion) by water, by alcohol, by acetic acid, by tartaric acid, by citric acid, or by saturated carbonate of ammonia.

The solvents, on the contrary, are far more numerous. It is dissolved, in considerable quantity, by muriatic acid, by nitric acid, by sulphuric acid, by phosphoric acid, and by oxalic acid.

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It is also dissolved readily by pure alkaline menstrua: by potash, by soda, by ammonia, and by lime-water. It is even dissolved by fully saturated carbonates of potash or of soda. Accordingly, these alkalies are not so convenient for the precipitation of this matter from acid solutions, as the carbonate of ammonia, which is not capable of redissolving the precipitate, though added in excess.

For a similar reason, the acids best suited for its precipitation from alkaline solutions, are the acetic and citric acids. But the tartaric acid may occasion an appearance of precipitation, by forming a supertartrate with the alkali employed.

The combination of this substance with acids, may be made to crystallize without difficulty, and they form slender spicula radiating from a centre, which readily dissolve again in water, unless they have been injured by being in any degree over-heated.

The muriatic salt is decomposed by the heat of boiling water, on account of the volatility of the acid, and the rest are easily destroyed by a greater excess of heat.

The salt formed by combination with nitric acid, does not yield oxalic acid, and does not become red, as the uric acid does, when similarly treated; but it turns brown, becoming gradually darker, till it is ultimately black.

When the combinations with alkalies are evaporated, they leave small granular crystals; but as I was desirous of rendering my experiments as numerous as a limited quantity would permit, the portion which I could employ in any one experiment was too small for me to attempt to determine the form of such crystals.

When a hot solution in potash was neutralized by distilled vinegar, the precipitate did not immediately take place, but formed gradually during the cooling of the liquor in minute crystals, some at the surface of the fluid, and others attached to the sides of the vessel. The only definite form which I could observe, was that of flat hexagonal plates, but I could discern nothing which enabled me to judge of the primitive form of the crystal. On the surface of the calculus belonging to Guy's hospital, some minute crystals may be discerned, of a different shape, being nearly cubic. And it is possible, that the hexagonal crystals may owe their figure to a small portion of alkali remaining in combination.

From the ready disposition of this substance to unite with both acids and alkalies, it would appear to be an oxide; and that it does, in fact, contain oxygen, is proved by the formation of carbonic acid in distillation. The quantity of oxygen present in the calculus is not, however, sufficient to

give it acid properties, for it has no effect on paper coloured with litmus.

I am therefore inclined to consider it as an oxide: and since both the calculi that have yet been observed have been taken from the bladder, it may be convenient to give it the name of *cystic oxide*, which will serve to distinguish it from other calculi; and as this is unlike any other term at present employed in chemistry, it is to be hoped that it will not be thought to require any alteration.

Since the period of my first essay on gouty and urinary concretions, the general results contained in it have been confirmed by others, and I believe are incontrovertible. But I am under the necessity of acknowledging a mistake in the analysis of the mulberry calculus, though not of much importance. An acid is mentioned to have arisen by sublimation, and it was supposed to originate from a partial decomposition of the oxalic acid. But since pure oxalate of lime yields no such sublimate, it most probably arose from the mixture of a small quantity of uric acid in the calculus then under examinatton.

In the analysis of the triple phosphate of mágnesia, there is another mistake of more consequence. In my selection from numerous experiments for ascertaining the presence of phosphoric acid, I gave the preference to one in which nitrate of mercury was employed, on account of the facility of extracting the acid from the phosphate of mercury, by heat alone. But since the whole of the phosphoric acid is not precipitated by nitrate of mercury, sulphate of magnesia will not be formed on the addition of sulphuric acid, and the magnesia cannot be obtained separate by the same process.

It may have been in consequence of this oversight, that a mistake on that subject has occurred in the succeeding volume of the Transactions.

A calculus is there described, which had been taken by Mr. Thomas from the bladder of a dog, and a series of experiments are related, from which it was inferred to consist of super-phosphate of lime, and phosphate of ammonia. But from the appearance of this calculus (which was exhibited to the Society at the time when the paper was read) I was much inclined to think that the nature of it was mistaken, and upon full consideration of the experiments, they did not appear to me conclusive.

I therefore obtained a portion of the calculus, and by the following process, the earth contained in it was proved to consist almost wholly of magnesia.

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It was dissolved, with the exception of a very small residuum, by distilled vinegar.

The whole of the phosphoric acid was then precipitated by acetate of lead, added to excess.

The liquor was then poured off, and sulphuric acid was added, which precipitated the excess of lead, and at the same time formed sulphate of magnesia in solution.

By evaporation to dryness, the acetic acid was removed, and by subsequent increase of heat, the sulphate of ammonia and excess of sulphuric acid were expelled.

The residuum being then dissolved in water, and the liquor suffered to crystallize by spontaneous evaporation, there remained a quantity of sulphate of magnesia, that weighed rather more than the quantity of calculus taken for the experiment.

It was evident, therefore, that in this instance, the calculus examined did not consist of super-phosphate of lime, and there is some reason to doubt, whether a compound, that is so very soluble in water, ever forms a part of urinary concretions.

Although the treatment of diseases is not in general a fit subject to occupy the time of this Society, there is nevertheless one suggestion, with respect to the prevention of calculous complaints, so nearly connected with my present subject, that I think it may deserve to be recorded.

Since the white matter contained in the urine of birds, which is voided along with their dung, has been remarked by M. Vauquelin to consist principally of uric acid, I have paid some attention to the different proportion in which this matter is voided by different species of birds, to see how far it accorded with the different qualities of their food. And I found that in the dung of the goose, feeding wholly on grass, the proportion did not seem so much as $\frac{1}{200}$ of the whole dung. In that of a pheasant kept in a cage, and fed on barley alone, it was about $\frac{1}{14}$ part. In that of a hen, having the range of a garden and farm-yard, and consequently procuring insects, and possibly other animal food, the proportion was manifestly much greater, and combined with lime. In the dung of a hawk, fed upon flesh alone, the quantity of matter voided in a solid state bears but a small proportion to the residuum of uric acid that is left by the urine when dry. And in the gannet, feeding solely on fish, I have observed the evacuations in some instances to be mere urine, for it contained no solid matter, excepting the uric acid.

It seems consequently deserving of inquiry, what changes

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might be produced in the urine of any one animal, by such alterations of diet, as its constitution would permit; for as far as any inference can be drawn from these varieties, which naturally occur, it would appear, that persons subject to calculi consisting of uric acid, as well as gouty persons, in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous.

IX. *On the Heat produced by Friction or Compression. By M.* Berthollet.*

Some years ago, with a view of more fully elucidating the origin of the heat occasioned by compression and friction, I formed the idea of examining by the help of a fly-press, the effects of compression on the metals: I applied to M. Gengombre for a press belonging to the Mint, and I requested Messrs. Pictet and Biot to assist me in my experiments. These were pursued for some time with all the precision that might be expected from such skilful coadjutors; but they were interrupted and abandoned before being brought to the point which I wished: I shall nevertheless present the results of some of these experiments.

I prepared pieces of gold, silver, copper, iron and bronze; all of the same dimensions, in order to submit them to the action of the press; but the experiments were chiefly made with those of silver and copper.

In order to determine the heat which the pieces of metal acquired by the shock of the fly-press, a thermometer placed horizontally was at first used; but it was afterwards found best to throw the piece of metal into a quantity of water sufficient to cover it. We had ascertained by preliminary experiments the relation which exists between the heat acquired by a certain weight of water, and the temperature of a given weight of each metal plunged into it: we thus estimated, by means of the heat which the water acquired on comparing its weight with that of the metal, the temperature to which the metal had been raised.

We submitted a piece of metal to the shocks of a fly-press put in motion by two men who were accustomed to this operation: we determined the heat acquired, and allowed the metal to return to a temperature precisely similar to that

* *Mem. d' Arcueil*, tom. ii. page 441.

that of the fly-press: we subjected it to a new shock; and performed a third operation with the same precautions.

In order that we might not be deceived as to the temperature acquired, we submitted to the thermometer a piece of metal similar to that which we had left some time under the fly-press, in order to ascertain the temperature of it precisely, and we noted the heat which the piece yielded that had been struck.

Experiment made with two Pieces of Copper.

		Increase of Temperature expressed in Degrees of the Centigrade Thermometer. Degrees.
First shock	First piece	9,69
	Second piece	11,56
Second shock	First piece	4,06
	Second piece	2,5
Third shock	First piece	1,06
	Second piece	0,81

The total quantity of heatextricated from the two prepared pieces is nearly equal; for, on adding the numbers, we find

For the first piece	14°,81
For the second piece	14 ,87

Experiment made with two Pieces of Silver.

		Increase of Temperature expressed in Degrees of the Centigrade Thermometer. Degrees.
First shock	First piece	3,44
	Second piece	4,56
Second shock	First piece	3,25
	Second piece	1,19
Third shock	First piece	1,50
	Second piece	1,12
	Total for the first piece	8,19
	for the second piece	6,37

Several other experiments gave analogous results.

But after three shocks of the fly-press, the pieces when struck again, either did not acquire a sensible heat, or exhibited it in a lower degree than in the third shock.

Gold afforded a heat still inferior to that of silver.

We afterwards proceeded to ascertain the relation which exists between the foregoing effects and the condensation of the volume of each metal. We took the specific gravity of the piece of gold at 8° of the centigrade thermometer.

meter. It was 19,2357: we re-melted it, and its specific gravity became 19,2240. After having polished it, its specific gravity was 19,2390: we then struck it, and its specific gravity became 19,2487.

Piece of silver	10,4667
Piece of re-melted silver	10,4465
Piece of silver struck	10,4838
Piece of copper	8,8529
Piece of copper, struck	8,8898
Piece of copper, struck a second time	8,9081

If we compare the foregoing experiments, we see that, independently of the difference of the specific gravities of the metals, gold undergoes in the compression caused by the shock a less condensation than silver, and the latter in its turn less than copper; and that the heat extricated is in proportion to the change of dimension. But we must compare the piece of copper with that of silver and of gold, regarding all three in the state in which they are when they are reduced into plates, *i. e.* when they have undergone the pressure of a flatting-mill, because copper cannot acquire its greatest dilatation by re-melting, which alters its surface. The re-melting, by producing a greater state of dilatation, diminishes the tenacity, which was owing to the compression of the metal, and it increases at the same time the proportion of caloric, which varies with the dimensions.

We struck into moulds some pieces of similar dimension to the above; but the heat extricated was less considerable, and the specific gravity received less increase than when the pieces struck were at liberty. This was certainly owing to the particles in the latter case coming more closely together by sliding over one another.

At the commencement of our experiments we made use of copper, and had, as we supposed, clearly ascertained that heat was extricated by compression; but, when all circumstances were alike, instead of heat, we had a production of cold. We verified the temperature of all the objects which could have any influence, and we found, by employing a very accurate thermometer of M. Pictet's, which we had used throughout the experiments, that the stamper of the fly-press was nearly half a degree below the temperature of the copper: we directed one experiment towards this object, and we ascertained that the communication of temperature takes place in a much more rapid manner by means of the blow and of compression, than when the bodies are simply

simply in contact. Since then, we took every necessary precaution in order that the fly-press and the metallic pieces should be at the same temperature before making the fly-press act.

It results from what precedes, that the heat which is produced by compression in bodies which do not undergo any chemical change, is merely owing to the changes of dimension which these bodies undergo; and when the dimensions can no longer be diminished, the shock, however violent does not cause any heat: solids then become similar to liquids, which may undergo violent and repeated shocks without any change in temperature: for it appears to me to be natural to attribute the small extrication of heat which we have been able to observe in the pieces which had undergone three operations, either to a small condensation which might still be produced in them, or to the effects of the elastic particles of the fly-press, which had been able to re-adjust themselves after the shock. 2dly, That the communication of the heat takes place much more rapidly by a strong compression than by simple contact: from which it follows, that in our experiments we have been able to obtain but a small part of the effects of the extrication of heat produced by compression; but this part ought to be in relation with the total effect.

X. *On the Parts of Trees primarily impaired by Age. In a Letter from* T. A. Knight, *Esq. F.R.S. to the Rt. Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.**

My dear Sir, In the first communication I had the honour to address to you, (it was in the year 1795,) I stated the result of many experiments on grafted trees, from which I inferred that each variety can be propagated with success, during a limited period only; and that the graft, or other detached part of an old tree, or old variety, can never form that which can with propriety be called a young tree.

I have subsequently endeavoured to ascertain which, amongst the various organs that compose a tree, first fails to execute its office, and thus tends to bring on the incurable debility of old age; and the result of the experiments appears sufficiently interesting to induce me to communicate an account of them to you.

Whatever difference exists between the functions of animal

* From the Philosophical Transactions for 1810, Part II.

mal and vegetable life, there is a very obvious analogy between some of the organs of plants, and those of animals; and it does not appear very improbable, that the correspondent organ, in each, may first fail to execute its office; and satisfactory evidence of the imperfect action of any particular organ can much more easily be obtained in the vegetable than in the animal world. For a tree may be composed, by the art of the grafter, of the detached parts of many others; and the defective, or efficient, operation, of each organ, may thus be observed with the greatest accuracy. But such observations cannot be made upon animals; because the operations necessary cannot be performed; and therefore, though there would be much danger of error in incautiously transferring the phænomena of one class of organized beings to another, I conceive that experiments on plants may be, in some cases, useful to the investigator of the animal œconomy. They may direct him in his pursuits, and possibly facilitate his inquiries into the immediate causes of the decay of animal strength and life; and on a subject of so much importance to mankind, no source of information should remain unexplored, and no lights, however feeble, be disregarded.

Naturalists, both of ancient and modern times, have considered the structure of plants, as an inversion of that of animals, and have compared the roots to the intestines, and the leaves to the lungs, of animals; and the analogy between the vegetable sap, and animal blood, is very close and obvious. The experiments also, of which I have at different periods communicated accounts to you, supported by the facts previously ascertained by other naturalists, scarcely leave any reasonable grounds of doubt, that the sap of trees circulates, as far as is apparently necessary to, or consistent with, their state of existence and growth.

The roots of trees, particularly those in coppices, which are felled at stated periods, continue so long to produce, and feed, a succession of branches, that no experiments were wanted to satisfy me, that it is not any defective action of the root which occasions the debility and diseases of old varieties of the apple- and pear-tree; and indeed experience every where shows, that a young seedling stock does not give the character of youth to the inserted bud or graft. I however procured plants from cuttings of some very old varieties of the apple, which readily emit roots; and these plants at the end of two years were grafted, about two inches above the ground, with a new and very luxuriant variety of the same species. These grafts grew very freely, and the

roots

roots themselves, at the end of four or five years, probably contained at least ten times as much alburnum, as they would have contained had the trees remained ungrafted. The roots were also free from every appearance of disease or defect.

Some crab-stocks were at the same time grafted with the golden pippin, in a soil where the wood of that variety rarely lived more than two years; and I again grafted the annual shoots of the golden pippin with cuttings of a young and healthy crab-tree; so as to include a portion of the wood of the golden pippin between the roots and branches of the native uncultivated species, or crab-tree; and in this situation it grew just as well as the wood of the stock and branches. Some branches also of the golden pippin trees, which I mentioned in my former communication of 1795, being much cankered, were cut off about a foot above the junction of the grafts to the stocks, and were regrafted with a new and healthy variety. Parts of the wood of the golden pippin, in which were many cankered spots, were thus placed between the newly inserted grafts, and the stocks; and these parts have subsequently become perfectly free from disease, and the wounds, previously made by canker, have been wholly covered with new and healthy bark. These facts, therefore, satisfied me, that the debility and diseases of old varieties of fruit of this species, did not originate in any defective action of the bark or alburnum, either of the root, or of the stem and branches; and my attention was consequently directed to the leaf and succulent animal shoot.

A few crab stocks were grafted with cuttings of the golden pippin, in a situation and soil where I had previously ascertained that the wood of the golden pippin rarely remained in health at the end of a second year; and, as soon as the annual shoots had acquired sufficient growth and firmness, numerous buds of a new and luxuriant variety of apple, which had recently sprung from seed, were inserted in them. During the succeeding winter, the natural buds of the golden pippin branches were destroyed, and those inserted suffered alone to remain; and as soon as the leaves of these had unfolded, and entered on their office, every symptom of debility and disease disappeared in the bark and wood of the golden pippin; and each continued to perform its office, just as well as the wood and bark of the young seedling stocks could have done under similar circumstances. I made nearly the same experiments on the pear-tree, and with the same result.

I have

I have endeavoured, in several former communications, to prove that the sap of plants circulates through their leaves, as the blood of animals circulates through their lungs; and I have not subsequently found any facts, in the writings of other naturalists, or in my own experiments, which militate against this conclusion. I have also observed, that grafted trees, of old and debilitated varieties of fruit, became most diseased in rich soils, and when grafted on stocks of the most vigorous growth; which has induced me to suspect, that in such cases more food is collected, and carried up into the plant, than its leaves can prepare and assimilate; and that the matter thus collected, which would have promoted the health and growth in a vigorous variety, accumulates, and generates disease in the extremities of the branches and annual shoots, whilst the lower part of the trunk and roots remain, generally, free from any apparent disease. I am, therefore, much disposed to attribute the diseases and debility of old age in trees, to an inability to produce leaves, which can efficiently execute their natural office; and to some consequent imperfection in the circulating fluid. It is true that the leaves are annually reproduced, and therefore annually new: but there is, I conceive, a very essential difference between the new leaves of an old, and of a young variety: and in support of this opinion, I shall observe, that the external character of the leaf of the same variety at two, and at twenty years old, is very dissimilar; and it therefore appears not improbable, that further changes will have taken place at the end of two centuries *.

If these opinions be well founded, and the leaves of trees be analogous to the lungs of animals, is it very improbable that the natural debility of old age of trees and of animals, may originate from a similar source?—This is a question, upon which I am not by any means prepared to give an opinion: but I believe it will very generally be admitted, that the human subject is best formed for long life, when the chest is best formed to permit the lungs to move with most freedom. I have also long and attentively observed

* The leaf of a seedling apple or pear-tree, when the plant is very young, is generally almost wholly free from the pubescence or down, which subsequently appears on its under surface; and which Bonnet and M. Mirbel have supposed to increase its surface and powers. But I feel little disposed to adopt this hypothesis, having observed that the leaves of some new varieties of the apple, which have sprung from seeds of the Siberian crab, have both surfaces nearly equally smooth; and that these varieties grow faster, and bear heavier crops of very rich fruit, than any others, without being exhausted or injured.

amongst

amongst our domesticated animals, that those individuals longest retain their health and strength, and best bear excessive labour and insufficient food, in which the chest is most deep and capacious, proportionately to the length of current the circulating fluid has to run; and the same remark will, I believe, be generally found applicable to the human species. I am, my dear sir,

With great respect, sincerely yours,

Downton, Feb. 26, 1810. THO. AND. KNIGHT.

XI. *Extract of a Letter from the Rev.* JOHN BRINKLEY, *D.D. F.R.S. Andrew's Professor of Astronomy in the University of Dublin, to the Rev.* NEVIL MASKELYNE, *D.D. F.R.S. Astronomer Royal, on the annual Parallax of α Lyræ.**

I HAVE now had sufficient experience of my eight-feet circle, to be highly satisfied with it, and have arrived at one conclusion, that it is of importance in astronomy.

My observations on α Lyræ for the purpose of discovering an annual parallax now amount to 47 in number: viz. 22 near opposition, and 25 near conjunction, and the mean of these gives a result of 2″,52 as the parallax of the annual orbit for that star, and I have no doubt that it exceeds 2″.

My observations of different circumpolar stars, and of the same star in different states of the thermometer, seem to require a small alteration in the numbers of Dr. Bradley's formula for refraction.

The formula so altered is

$$\text{Refr.} = 56'',9 \times \text{tang.} \left\{ \text{Zen. dis.} - 3,2\ \text{Refr.} \right\} \times \frac{\text{height of barom.}}{29,6} \times \frac{500}{450 + \text{ther.}}$$

By means of this formula, the observations of circumpolar stars considerably distant, give the same co-latitude to a great degree of exactness.

XII. *On the Alteration which Air and Water produce on Meat. By* M. BERTHOLLET.†

I BOILED some beef, renewing the water until the liquor no longer gave any precipitation with tannin: I then suspended it in a glass cyclinder filled with atmospheric air, and which I placed on a plate filled with water: in a few days

* From the Philosophical Transactions for 1810, Part II.

† *Mem. d'Arcueil,* tome i. p. 333.

days the oxygen was changed into carbonic acid; the interior of the cylinder was infected with a putrid smell; the beef that had been boiled gave once more an abundant precipitation with tannin: the ebullition was repeated until the water was no longer disturbed by the tannin: the beef had then almost entirely lost its smell, and it was put again into the apparatus.

The operation was repeated several times, when the following were the results:—

The alteration of the atmospheric air and the disengagement of the putrid smell gradually slackened: the quantity of gelatine formed became progressively smaller: the water upon which the vessel rested gave only feeble indications of ammonia. When I finished the operation there was no longer any putrid smell; but a smell similar to that of cheese, and in fact the animal substance which now preserved scarcely any fibrous appearance, had not only the smell, but precisely the taste of old cheese.

I distilled, separately, an equal weight of beef and of Gruyere cheese, making use of two bell-glasses, each of which communicated with a tube inserted in water: the operation was conducted so as to decompose, as much as possible, the two substances, and to retain all the ammonia which was set free: I compared the quantities of ammonia; that which was furnished by the cheese was to that of the beef nearly in the ratio of 19 to 24: hence it appears that it is one of the distinctive characters of the caseous substance to contain less azote than meat does.

If we may be permitted to draw any inferences from the foregoing imperfect experiments, we may conclude:

1st. That the gelatine which we may obtain from an animal substance is not completely formed in it; but that when this substance has been exhausted by water, it may be once more formed by the action of the air, the oxygen of which is combined with the carbon, while a portion of substance formerly solid becomes gelatinous, as a solid vegetable part becomes soluble by the action of the air.

We must, however, remark, that the property of precipitating with tannin belongs to substances which have very different properties in other respects: I found that the decoction of Gruyere cheese formed an abundant precipitate with tannin.

2d. That azote enters into the composition of the putrid gas, forming without doubt with hydrogen a combination of an equilibrium less stable than ammonia, or rather an intermediary combination; but when its proportion is diminished

minished to a certain point, it is more strongly retained by the substance, and it ceases to produce putrid gas. This substance, which the putrid odour characterizes, seems rather to be a very evaporable combination, allied to all the gases like the other elastic vapours, than a permanent gas.

3d. Since the caseous part has less azote than most other animal substances, we may conjecture that during life this part is animalized more and more by acquiring a greater proportion of azote and hydrogen: this may be explained by the more intimate combination of oxygen and hydrogen which enter into its composition, and by a separation of the carbon by the act of respiration; so that the last term of chemical action during life has urea for its product, according to the opinion of M. Fourcroy *.

XIII. *On the Purification of Platina. By* M. Descostils †.

The only method hitherto known of purifying platina, is to dissolve it in the nitro-muriatic acid, and to precipitate it by ammoniacal salt. The decomposition of the triple salt afterwards takes place by simple heat, and we obtain platina in the metallic state.

This process is attended with two inconveniences:—the first, is the great quantity of acid necessary for the solution of the platina in grains: the second, and the principal one, is the union which the platina contracts with iridium in its precipitation by ammoniacal salt. This union can no longer be destroyed but by repeated solutions and precipitations, which successively give a salt more and more exempt from iridium on account of the trifling action which the acids exercise on this last metal, and of the greater solubility of its saline combinations with respect to those of platina. We may, indeed, abridge these operations, by employing only the orange-coloured salt which we obtain from the precipitation of the solution of platina in grains a little diluted with water; but then we lose a portion of the platina which remains in the mother waters, and this loss would be important if we operated on large quantities.

I endeavoured to remedy these two inconveniences by introducing some modifications, founded upon properties already known indeed, but which had not been brought into play. The following are the modifications to which I allude:

* *Syst. des Conn. Chimie*, tome x. p. 165.
† *Mém. d'Arcueil*, tome i. p. 370.

Instead of directly dissolving the ore of platina in the acids, I begin by melting it with zinc*. The alloy is formed with facility, and even with an extrication of heat and light, as has been observed by Lewis. The heat produced by a common reverberating furnace is sufficient for determining this combination. We must first heat the zinc; and as soon as it is melted, gently pour the platina above it: we then cover the crucible, and increase the fire by placing the dome on the furnace, and adapting to it a pipe about a metre in height. Half an hour afterwards, if the mass be not very considerable, we take off the crucible: a part of the zinc is vaporized in this operation, and consequently it is lost; but this is inevitable, if we wish to have a homogeneous combination.

The alloy which we obtain is of a garyish white, a little grained in its fracture, and very easily pulverized. We reduce it into fine powder, and attack it with the sulphuric acid diluted in two or three times its weight of water. When the action of the acid diminishes, we must restore it by the help of heat; and when it ceases entirely, we decant the liquor and pour new acid on the residuum; we continue in this manner until the acid no longer produces any effect. By operating in this manner we easily obtain sulphate of zinc from the liquors first decanted; the last may be reserved in order to attack new quantities of alloy.

When the sulphuric acid alone exercises no more action, we add a small quantity of nitric acid, which determines the solution of a new quantity of zinc. We may then employ a sulphuric acid more concentrated. In this last case it takes up a little platina and palladium, but we may recover these metals by the green sulphate of iron and sal ammonia, and afterwards purify the sulphate of zinc with metallic zinc.

When the acid can dissolve no more, we decant the liquor and wash the residuum†. It is then easily dissolved in the nitro-muriatic acid‡. The proportion of the nitric and muriatic acids which appeared to me to be the most convenient, is that of one part of the former to three of the latter.

* I employed four parts of zinc to one of platina; but I am of opinion that this proportion may be greatly diminished.

† This residue burns with facility at a very slight heat; and even if we have employed a smaller proportion of zinc, the residue detonates like gunpowder. The muriatic acid takes this property from it.

‡ That which I obtained from an alloy of four parts of zinc and one of platina in grains, an alloy which by fusion had lost one part of the zinc, only required for its solution a little more than four times the weight of the platina in nitro-muriatic acid.

latter. I ought to observe, that there is a great advantage in not making the mixture before employing them. It is best first to pour the nitric acid on the metal, and afterwards gradually add the muriatic acid, until this addition does not produce any effect. It is also advantageous to employ the acids in small quantities, by decanting the solution before pouring in new nitric acid.

After the solution is terminated, we allow the liquor to rest some time in a high vessel of a small diameter. When it is very clear, we decant it with a narrow syphon, in order to separate the black powder which remains after the solution of the platina, and evaporate the liquor to perfect dryness. If we afterwards dissolve the residue in a small quantity of water, and allow it to rest for 24 hours, almost the whole of the gold which was contained in the ore is deposited at the bottom of the vessel in the metallic state. We decant again; and if we wish to obtain the last portions of palladium, we add to the liquor a little prussiate of mercury, which, according to Dr. Wollaston, precipitates this metal completely. We then filter, and pour into the solution pure carbonate of soda, until the precipitate which is formed no longer increases: the effect of this addition is to form a triple salt of soda and platina, which, according to M. Proust, is not decomposed by the carbonate of soda added in the proper proportion, while the iron is precipitated. We separate this last by the filter, or, what is preferable, allow it to rest, and then decant it; we wash the deposit several times, and mix the liquors; but as the last washings contain very little platina, we may put them aside in order to precipitate the metal from them by sulphuretted hydrogen, or by a hydro-sulphuret. The roasted precipitate may afterwards be treated with the crude ore in a subsequent operation.

If we are afraid that the ferruginous deposit does not retain any platina, we may ascertain it by dissolving a small portion in concentrated muriatic acid, and by adding to the solution muriate of ammonia. If the triple salt which is deposited seems to be abundant, we treat the whole of the ferruginous deposit in the same manner.

The liquor which contains the triple salt of platina and of soda ought to be slightly acid. We must add to it carbonate of soda until it becomes distinctly alkaline. On leaving it exposed for some time to the air, the iridium would be separated in the form of a green sediment; but in order that this separation may take place more quickly

and more completely, the solution ought to be slightly heated, *i.e.* to 50 or 60° of the centigrade thermometer. The iridium is then deposited in abundance, as I have shown in my memoir on the influence of this metal in the colouring of the salts of platina *. In order that the separation may be as complete as possible, the solution must not be too much concentrated nor too alkaline.

When the deposit no longer increases, we must filter and pour into the liquor, after it is cooled, as much muriatic acid as is necessary to make it again become very sensibly acid. We then precipitate with sal ammonia; and when the precipitation is complete, we filter and wash the triple salt several times with small quantities of water.

The liquors still contain platina and a little iridium. We precipitate these metals by a hydro-sulphuret, and treat the precipitate as above described.

The triple salt which we obtain by this process, if it is pure, ought to be of a clear golden yellow. The nitric acid which dissolves it by ebullition ought not to give it any more intenseness of colour than that which ought to be produced by the greatest volume of crystals which are formed again upon cooling. There is besides an easy method of ascertaining the smallest portion of iridium. It consists in reducing by heat a small portion of the salt which we wish to examine, dissolving the residue by the nitro-muriatic acid, (or we may dissolve directly the triple salt by this acid, which in this case decomposes the ammonia,) and again precipitating the metal by sal ammonia, taking care only to employ the necessary quantity. We separate the triple salt by the filter, and evaporate the mother-waters. If the salt which they furnish is of a clear yellow, we may be assured that there is no iridium in that which we are examining. If, on the contrary, the salt is red, it is a proof of the metal being there; and we must treat in the same manner the whole of the triple salt, if we wish to purify it entirely.

The reduction by heat of a small quantity of triple salt requires some precaution. If we operate in a crucible, it frequently happens that the jets of vapour which rise, carry up with them great part of the salt: I prefer drying it first in a porcelain capsule, and finish the reduction in a stone retort. I afterwards wash the metallic sponge, until it no longer contains any thing soluble: I even boil it with a little sulphuric acid, and after having well washed it, I re-

* *Annales de Chimie*, tome xlviii. p. 170.

dissolve

dissolve it. In this state of tenuity the platina requires but little acid. I afterwards precipitate with the ammoniacal muriate; I wash the salt repeatedly with small quantities of water, and obtain by its reduction the platina in its greatest known state of purity.

XIV. *Notices respecting New Books.*

Elements of Chemistry. *By* J. Murray, *Lecturer on Chemistry, and on Materia Medica and Pharmacy, Edinburgh.* Two Vols. 8vo. pp. 1040, with three Plates.

The author has not announced this work as the second edition of the Elements of Chemistry, which he published some years ago; as, from the rapid progress of the science, it has been necessary, as he informs us, to write it nearly anew. "Its object, however, is the same,—to give such a view of chemistry as shall convey a just knowledge of its leading principles, and more important facts, without including the discussion of controverted opinions, or the statement of those minute details which have with propriety a place in a systematic work."

Modern chemists in forming their systems of chemical classification have in general followed the synthetic arrangements: Mr. Murray, on the contrary, has adopted the analytic, and we think that in this he has shown his judgement; for we cannot conceive any thing more decidedly calculated to retard the progress of a tyro, than attempts to make him comprehend the nature of simple substances which have never yet been presented, *per se*, to the cognisance of sense. The most direct introduction to a knowledge of chemistry would, in our opinion, be, to commence with such facts as present themselves daily and are more or less known to all—as the boiling of water, the causes which co-operate, the effects that accompany it, and the processes to which it gives rise; combustion, the accompanying phænomena as its respects the changes produced on the air, production of carbonic acid gas and azote, the emission of light and heat; lime-burning conducted in close vessels, the gas produced, its identity with that obtained by the combustion of charcoal, solubility and precipitation of lime, &c.&c.&c. By some such method as this the pupil might be made to witness the production of the various agents employed in analysis and combined by synthesis, nor would he feel himself compelled to take any thing upon trust, in the

the hope of future evidence demonstrating its truth—a mode of procedure which always tends to enfeeble the mind and to induce improper acquiescence in the mere dicta of authority, without exercising the judgement.

The classification followed by Mr. Murray is the following:—Part I. Of the general forces productive of chemical phænomena—attraction of aggregation—chemical attraction or affinity; of repulsion, and the powers by which it is produced—caloric—light—electricity and Galvanism. Part II. Of the chemical properties and relations of individual substances; of atmospheric air and its principles—oxygen—nitrogen; of water and its base—hydrogen; of alkalis and their bases; of earths and their bases; of acids and their bases; of metals; of the native combinations of acids, earths, metals, and inflammables—mineral compounds; of vegetable compounds; of animal compounds.

We cannot but think that in the present state of the science of chemistry, the alkalis and the earths should have been classed with the metals; but perhaps the author was afraid of being thought an innovator. His work, however, presents all the modern discoveries, detailed with considerable precision and perspicuity.—As a specimen of the author's style, we extract what he says

"*Of the Radiation of Caloric.*

"Besides the caloric, which a body in cooling communicates to the matter with which it is in contact, a portion is thrown from its surface in right lines moving with great velocity, capable of being rendered sensible at a considerable distance, and obeying the same laws of motion as the rays of light. This forms the radiation of caloric. It was observed more than a century ago, by Mariotte, and also by Lambert; and within a later period it has been investigated by Scheele, Saussure, and Pictet, Herschel, and Leslie.

"The experiment in which this radiation of caloric is best displayed, consists in placing a hot body, as a heated ball of iron, in the focus of a concave metallic mirror, opposite to which, at the distance of 10 or 12 feet, is placed a similar mirror, having the ball of an air thermometer in its focus*. The moment the hot body is introduced, the opposite thermometer indicates elevation of temperature, the air in its ball being expanded, and pressing on the liquid so

* The differential air thermometer is well adapted to show these effects, and to allow all the facts with regard to the radiation of caloric to be determined with much accuracy. It was applied to this purpose by Mr. Leslie.

as

as to cause it to descend. If the hot body be withdrawn, or a skreen be interposed between the mirrors, the temperature falls, and the liquid in the thermometer rises to its former height. In this experiment, there has been projected a calorific matter from the heated body, on the surface of the mirror in the focus of which it is placed; this has been reflected in right lines from the surface of this mirror to the one opposed to it, it is again reflected from the surface of that mirror, and is collected in its focus, where it produces a heating effect.

"The effect is similar with a single mirror. If a hot body be placed before its concave surface, at the distance of a few feet, and a thermometer be placed in its focus, rays of caloric are projected from the hot body, and are reflected from the surface of the mirror on the thermometer, producing elevation of temperature.

"That it is not the contiguity of the hot body to the thermometer that produces the effect in these experiments, is well shown, not only by the distance at which it happens, but also by moving the thermometer a little out of the focus, even nigher to the heated surface, when its temperature, if it had been previously raised, immediately falls.

"The rise of temperature produced by this radiation is greater, the hotter the body is from which it takes place. In using the apparatus of the double mirrors, and placing in the focus of one of them a ball of iron, two inches in diameter, at an obscure red heat, the elevation produced in a thermometer, in the focus of the other mirror, at the distance of 12 feet, is equal to about 20 degrees of Fahrenheit's scale. From a glass matrass, containing about two ounces of water boiling, it does not exceed three degrees. From burning charcoal, the heat is such, that it can set fire to a burning body at the distance of several feet.

"The velocity with which radiant caloric moves, is not capable of being measured at any distance at which we can make the experiment. In an experiment by Pictet, the effect appeared instantaneous at the distance of 69 feet. It appears to pass through the atmosphere without interruption; nor, according to Scheele's experiments, is its direction changed by a current of air. It is stopped, however, by liquids, even the most transparent.

"Glass also intercepts a large portion of it. If a plate of clear glass be interposed half way between the two mirrors, a hot body being in the focus of the one, and the ball of a thermometer in the focus of the other, the effect on the thermometer is nearly entirely intercepted. The rays of caloric

caloric thrown on the glass, instead of passing through it, are absorbed by it. This result affords a method of separating the rays of caloric from the rays of light, when they accompany each other. Thus, if a burning candle be placed in the focus of the mirror, and a plate of glass interposed, a luminous image is formed on the ball of the thermometer in the opposite focus, from the light passing through the glass; but the calorific effect is greatly diminished, by the rays of caloric being arrested,—a fact which shows well the essential difference between radiant caloric and light.

"Some bodies are more disposed to absorb radiant caloric than others, and hence are much more heated by it. Scheele observed, than when a glass mirror is used instead of a metallic one, the heat is not reflected, but is absorbed and retained by the glass; and the result is similar, if a metallic mirror have its surface blackened. Pictet found, that when the glass bulb of the thermometer is blackened, it is considerably more heated than when it is clean. But if the bulb be covered with tinfoil, the reverse happens, or the elevation of temperature is much less than when the glass bulb is opposed to the mirror.

"The power of reflecting the rays of caloric is of course the reverse of the absorbing power. Metals reflect most perfectly, hence the calorific effect is greatest in these experiments when metallic mirrors are employed; it is less with a glass mirror, and is scarcely sensible when the surface is blackened. It is, for the same reason, least when the ball of the thermometer has a metallic surface, is greater when of glass, and still greater when blackened.

"An important difference exists among bodies in the power of radiating caloric, the quantity thrown from different kinds of surfaces at the same temperature being very different. For the knowledge of this, we are indebted to Mr. Leslie. The apparatus he employed to determine it is very simple. It is a canister of tinned iron in the form of a cube, the side being six or eight inches square; this is filled with hot water, a thermometer being inserted in it, to show the temperature during the continuance of the experiment. The sides of the canister are variously prepared; one for example is blackened, another is covered with paper, a third has a plate of glass applied to it, and the fourth is left clean. When thus prepared, it is placed before the concave surface of a mirror of tinned iron, at the distance of three or four feet; the ball of the differential thermometer being adjusted to the focus. All these surfaces being equally under the influence of the hot water in the canister, are at the

the same temperature, and their comparative powers, in radiating caloric at a given temperature, can be ascertained. In this respect they differ widely: as an average it may be stated, that the calorific effect from the blackened surface being 100, that from the paper is 98, from the glass 90, and from the clean metal not more than twelve.

"It thus appears, that those surfaces most disposed to absorb radiant caloric, when it is thrown upon them, are those likewise most disposed to radiate it when they are at a high temperature, and the absorbing and radiating property are opposed to the reflecting power.

"It is an interesting question on this subject, Does radiant caloric suffer refraction? This was attempted to be determined by Pictet, but the experiment was so imperfectly performed, that no conclusion could be drawn from it. Herschel found reason to conclude, that rays of caloric exist in the solar beam, apart from the rays of light, and that these are separated when the beam is decomposed, by being passed through the prism, the calorific rays being thrown beyond the red ray; this, if the experiment were accurate, proved, that these calorific solar rays at least are subject to refraction. He further submitted to experiment, the radiant caloric projected from heated bodies; and he found them to be refracted by a lens, and in the spot where they were collected by the refraction, to produce a heating effect.

"Mr. Leslie observed, that a considerable aberration happens in the reflection of heat; hence, when reflected from a mirror, the maximum of heat is not in the true focus, but is found to be considerably nearer to the surface of the mirror.

"If the experiments of Herschel be admitted as accurate, they establish the important discovery, that radiant caloric exists in the rays from the sun, and that on this depends their heating power. In decomposing the solar beam by transmission through a triangular glass prism, it is resolved into different coloured rays, and these Herschel found were possessed of different degrees of heating power, the violet ray, which is the most refrangible, and which bounds the coloured spectrum on one side, being least powerful in exciting heat; and the calorific power, increasing towards the other side, bounded by the red ray, which far exceeds the others in heating power.

"All this, however, might be considered as arising from diversity of heating power in the visible rays of light; but Herschel further found, that calorific rays which produce no illumination exist in the solar beam, which being less refrangible

refrangible than any of the rays of light, occupy a space beyond the red ray, when the entire beam is decomposed by the prism. In this space, to the extent even of half an inch beyond the visible light, the heating power is actually greater than in the space occupied by the red ray, which of any of the coloured rays produces the greatest heat, and it can be traced even to the extent of an inch and a half. This appears to prove the existence of rays of caloric in the solar ray, which, from being less refrangible, are capable of being separated from the visible light; and if the accuracy of the experiments be admitted, scarcely any other conclusion can be drawn. Herschel further infers, that the heating power of the different coloured rays does not belong to the light of these rays, but depends on rays of caloric associated with them, there being, according to his hypothesis, rays of caloric as well as of light of different degrees of refrangibility, and the former being therefore spread over the space occupied by the prismatic spectrum as well as the latter. This being more intimately connected with the chemical history of Light, will be afterwards more fully considered. In one respect the calorific rays in the solar beam differ from those projected from heated bodies; they pass without interruption through transparent media: this is evident indeed from the intense heat produced in the focus of a lens when the rays of the sun have been transmitted through it. It was ascertained, too, with more accuracy by Herschel, the solar calorific rays, whether associated with light or separated from it, passing through transparent substances and producing heat; while the radiant caloric from heated bodies is almost entirely arrested.

"An important subject of inquiry is still to be considered; What is the nature of radiant caloric, or what theory can be given of the phænomena it displays?

"These phænomena appeared to prove the existence of a subtle calorific matter, projected from heated bodies, capable of moving in right lines with velocity, and obeying laws of motion similar to those of light; and this conclusion was accordingly generally drawn and received. Mr. Leslie, however, advanced a different hypothesis; the apparent calorific emanation he supposed to be propagated entirely by the medium of the air. The heated surface, according to his view, communicates increased temperature to the portion of air in contact with it; this layer of air is expanded, and presses on the portion immediately before it. This is successively, but rapidly renewed; a chain of undulations is propagated from the heated surface to the mirror, reflected and

and concentrated in its focus, and each pulsation being accompanied, according to the hypothesis, with a discharge of the caloric by which the expansion exciting it had been produced; the whole is transported with the velocity of these undulations, and the calorific effect is obtained where they are concentrated on a solid substance. The degree of heat excited will, of course, be greater as the temperature of the surface communicating it is greater. And the diversity in the effect from different kinds of surface at the same temperature, Mr. Leslie explains by the hypothesis, that they admit of a more or less perfect contact of the atmospheric air; those with which the air comes into closest contact, and this, of course, is supposed to be the case with the blackened surface, communicating the largest quantity of caloric in a given time; and for a similar reason, the same surfaces will be those most disposed to receive caloric, and will therefore be those most heated by this kind of communication.

"This hypothesis rests principally on certain facts observed by Mr. Leslie with regard to the effect of skreens interposed between the hot body and the mirror on the calorific radiation. It had been observed, that when a plate of glass is interposed, the effect on the thermometer in the focus is greatly diminished. Mr. Leslie found that this is much dependent on the distance at which the glass plate is placed from the heated body. In the apparatus with the single reflecting mirror, if the plate of glass be placed at about two inches from the blackened surface of the canister, a rise in the thermometer is produced equal to about one-fifth of what would be produced by the same surface, the glass being withdrawn; if further removed from the heated surface, the effect on the thermometer diminishes; and when it is removed about a foot, it does not amount to one-thirtieth of what it is in the first position. Mr. Leslie further found that the effect was very different with skreens of different kinds; with one of paper interposed, it did not differ much from that with the glass, but if a metallic skreen was used, though extremely thin, as for example gold leaf, the effect on the thermometer was completely intercepted.

"These results cannot be explained on the supposition that these skreens operate by intercepting more or less the calorific radiation, some doing so completely, others more imperfectly; for, were this the case, the action of those which allow a certain degree of heating effect to be produced on the thermometer ought to be the same at whatever distance it is placed from the heated surface, while the fact

is,

is, that it is much dependent on its contiguity to it. They therefore, Mr. Leslie conceives, establish the conclusion, that these skreens, in every case, arrest the radiant caloric, and that where any effect is produced on the thermometer, this is to be ascribed to the interposed skreen acquiring heat, and being thus enabled to display the same action as a similar radiating surface would do at the same temperature. Accordingly, when a skreen is employed which is not much disposed to receive radiant caloric on the one hand, or to radiate it on the other, as one of metal, no effect is produced; or if the skreen is such, that its temperature cannot be raised, as is the case for example with a plate of ice, there is also no effect; but, if the skreen be of a substance disposed both to absorb and radiate caloric, as in the case with glass or paper, then a certain effect will be produced, the side next to the hot body arresting the calorific radiation and having its temperature raised, and the other radiating proportional to this rise of temperature,—and this, of course, will be greater the nigher the skreen is to the heated body.

"Now this effect of these interposed skreens, Mr. Leslie further conceives, can only be explained on the supposition that the air is the vehicle of the communication, as already explained, the skreen arresting the chain of pulsations, and acquiring in its turn to a certain extent the power of transmitting these pulsations with the accompanying discharges of caloric from the other surface; and on this assumption in a great measure rests his hypothesis.

"It is one which does not appear necessarily to follow, and it is perhaps equally conceivable on the hypothesis of the existence of rays of caloric, that these may be arrested by the skreen, its temperature may be raised, and corresponding rays be projected to a certain extent from its other surface: it must, in fact, be supposed, that the interposed skreen receives caloric at the one surface, and communicates it from the other, whether the caloric be supposed to be propagated by pulsations in the atmosphere, or by actual projection of calorific particles; and in either hypothesis, those most disposed to receive it, and again to discharge it, will be those which will admit of the greatest heating effect being produced on the thermometer.

"There is also some obscurity with regard to the principle of Mr. Leslie's theory; for admitting, that a chain of vibrations, such as he supposes, may be established in an elastic medium from a heated surface, it is not very obvious how each pulsation should be accompanied with a discharge of the

the heat by which it had been excited. Or if there is any necessary connexion between these events, there remains the difficulty of accounting for the slow communication of caloric through elastic fluids. The theory, too, is incompatible with the results of the experiments of Herschel and Englefield; which, if they are admitted as accurate, establish the existence of a subtle calorific matter, capable of rapid projectile motion. These experiments, however, Mr. Leslie considers as altogether fallacious.

"It has sometimes been conceived that radiant caloric is a species of light. Dr. Hutton, assuming that the heating powers of the different species of visible light are not proportional to their power of exciting vision, supposed there might be a species of light capable of exciting temperature without exciting this sensation, and such he conceived to be the nature of radiant caloric. There appears little foundation for this hypothesis. So far as we can trace, radiant caloric has all the properties of caloric conveyed by slow communication, and the mere circumstance of its assuming a state of projectile motion, if it actually do so, is insufficient to identify it with light. It exerts none of the chemical agencies of light. And the very basis of the hypothesis is subverted; for, as is afterwards to be stated, it is uncertain if any of the rays of light apart from caloric have a heating power.

"It is an interesting object of investigation, What is the relation subsisting between those two modes in which caloric is discharged from bodies, that by radiation, and that by slow communication? There appears, in general, reason to infer, that those which at a given temperature give off most caloric by communication, discharge least by radiation, and *vice versâ*,—metals, for example, radiating imperfectly, while they yield caloric readily by communication, while glass is, with regard to these properties, precisely the reverse.

"An inquiry of equal importance is, What proportion does the caloric discharged by radiation from a body suffering reduction of temperature, bear to that given out by slow communication? The influence of each of these modes is established by numerous facts. That of slow communication is well shown by the different degrees of celerity with which a body cools, according to the conducting power of the medium with which it is in contact, or according as the conducting power is favoured by frequent renewal of that medium; as, for example, by the application of a current of air, or agitation in a liquid. The influence of radiation

diation is not less important, and has, in particular, been very clearly established by some very excellent experiments by Mr. Leslie on the celerity of cooling in vessels, which radiate caloric unequally; water, for example, cooling more quickly ina tin vessel coated with lamp-black than when clean, the coating, though diminishing the conducting power, more than compensating for this by increasing the radiating power.

"The proportion between the two must be considerably dependent on the temperature at which the estimate is made; for at high temperatures the cooling by slow communication will be accelerated by the more rapid current formed in the surrounding medium from the heated surface, while this can have no effect on the radiation. Mr. Leslie concludes from his experiments, that at low temperatures the heat lost by the direct communication is somewhat less, and at higher temperatures considerably greater than what is lost by radiation.

"The influence of these circumstances on refrigeration gives rise to some results rather singular, and to some practical applications of considerable importance. Thus water cools more quickly in a metallic vessel, the outside of which is blackened, coated with varnish, or even covered with linen, than when clean and polished. Hence, in conducting the process of artificial refrigeration, vessels with such coatings will allow it to be performed most quickly; for the same reason, where the object is to condense vapour or steam, as, for example, in applying this condensation to procure heat, the external surface of the tubes through which the steam passes ought to be painted or blackened; while, if it is of importance to prevent as much as possible the condensation, as in conveying steam, or applying its elasticity as a mechanical power, the external surface ought to be clean and bright."

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THIS Society assembled, after the holidays, on the 10th; and concluded the reading of Dr. Parry's paper on the means of curing certain nervous affections merely by pressure on the carotid artery; a method which he proposed several years ago, and has found effectual in almost every case.

The same evening a mathematical paper on the hyperbola

bola was laid before the society by the Rev. Mr. Helens; but it was of a nature not to be read.

A curious account of a child born in Wales without eyes, or rather without eye-balls, was communicated by Mr. Jones: a small round white ball is found in the place of the eye, and the tunica conjunctiva was perfect. The mother attributes this organic defect to a fright which she received when seven months gone with child.

A letter from Dr. Wollaston to Dr. Marcet was read, in which Dr. W. related his experiments formerly made with a view to ascertain the existence of sugar in the serum of the blood of diabetic patients. The result of a considerable number of experiments, as well as the imperfect attempts of Mr. Cruickshank and Dr. Rollo, convinced him of the non-existence of sugar in such serum. Dr. Marcet, at the instance of Dr. Wollaston, also made some experiments with the same view, and administered five grains of prussiat of potash (without danger) to a patient whose urine yielded a blue colour on the addition of iron.

On the 17th and 24th, a long paper by Mr. Macartney was read on the nature of vital heat. Mr. M. related the appearances exhibited in a great number of experiments on eggs and rabbits, made with a view to ascertain the origin and progress of animal heat in the young chick, &c. and from them concluded, that vital heat does not depend on respiration, and that it may exist in any form of matter. This conclusion, however, he expressed with extreme diffidence; and, as if either afraid of its validity or of the accuracy of his own experiments, partly wished to decline giving any opinion on a subject which he still deemed very complex and imperfectly understood.

XVI. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir, Mr. Cuthbertson's communication in your Magazine of October last, on the mode of increasing the charge of electrical jars and batteries, brought to my recollection a scheme entertained some years ago by the late Mr. Brooke of Norwich, (inventor of the electrometer mentioned in Mr. Cuthbertson's Practical Electricity, page 173,) for preventing the bursting of jars by spontaneous or other discharges:—whether it succeeded or failed, I know not. It was by previously coating the jars with writing-paper pasted on within and without, so as to form an intermediate surface between the glass and the tin-foil on each side. The

The same electrician once showed me at his house in Norwich some jars of very large dimensions, partially coated on each side with tin-foil in perpendicular stripes of about three quarters of an inch in breadth, and the like distance asunder, connected above by a band of foil passing round the glass, and at the bottom by radii from the upright stripes meeting in the centre. He at the same time assured me that he found this mode of coating to produce as high a charge, as if the tin-foil was laid on in the usual way.

If you think this information worthy of insertion in your valuable publication, it is much at your service.

I am, sir, your obedient servant,

Wells, Norfolk, Jan. 14th, 1811. JOHN HILL.

Mr. George Singer has recently discovered a new system of arrangement for the insulators employed in electrical apparatus, by which their insulation is preserved, without the necessity of wiping, through all the vicissitudes of atmospherical change.

An electrometer has been constructed on this principle, which has maintained its insulating power undiminished during the last three months, although exposed for that time (in a large room without a fire) to the vapour, &c. from various chemical processes. The security is indeed so great, that when the glass cylinder of the electroscope is even rendered *opaque* by a coating of precipitated vapour, the divergence is not perceptibly lessened.

A detailed account of this method of insulation, and an enumeration of the instruments to which it will apply, will be shortly submitted to the public.

LECTURES.

The following Spring Course of Lectures will commence the beginning of February.

At St. Thomas's Hospital.

Anatomy and the Operations of Surgery by Mr. Cline, and Mr. Astley Cooper.

Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's Hospital.

Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

Our Meteorological Table for January is unavoidably postponed till next Month.

XVII. *A Method for ascertaining Latitude and Time by means of Two known Stars. By M.* Delambre. *Translated from the* "Connoissance des Tems" *by Mr.* Firminger, *many Years Assistant Astronomer at the Royal Observatory, Greenwich**.

This problem is well known to astronomers, who resolve it by the plain rules of spherical trigonometry, applied to three connected triangles. Several geometers have thought it worth their while to take it into their consideration, and M. Gauss has just made it the subject of a dissertation, an extract of which has appeared in the Journal of Baron de Zach. Our readers will see with pleasure the new researches of the learned professor who holds so distinguished a rank among geometers and astronomers.

Besides endeavouring to render the solution of this useful problem more useful and commodious, M. Gauss's object has been to prove that, by a well understood use of analysis, the same results may always be obtained, as are more commonly sought for by purely geometrical considerations.

I was ever persuaded of the truth of this assertion; but I have also generally found that the demonstrations are longer and more painful; that a considerable degree of address is sometimes required to attain the end; and that often it is useful to know beforehand, and by another process, what is sought for, as otherwise we may be led into analytical combinations that produce nothing convenient: but though it may thus happen at times that trials are unproductive, they will also sometimes lead to unexpected theorems, of which much simpler synthetic demonstrations will afterwards be found.

The best way, in my opinion, would be not to exclude any thing; to mix analysis with synthesis, to set down the equations of the problem in an easy construction, to avail ourselves of all the formulas that an immediate inspection of the figure may furnish, and to endeavour afterwards, by analytical processes, to give those equations a form the most advantageous, and most applicable to the use of logarithms.

In order to set a proper value on the advantages of the analytical solution of a known problem, it is necessary first to compare it with the vulgar solution; but this last should be previously reduced to general formulas, whereby no other attention is required in the calculator but that of

* Superintendant of the late Right Hon Charles F. Greville's Plans for Education at Milford, in South Wales; Teacher of Astronomy, Geography, Navigation, and the various Branches of the Mathematics.

the algebraic signs. The two solutions are then applied to the same example, after which we are enabled to draw certain consequences.

This is what we are going to do:—we shall reduce the trigonometrical solution to general formulas: we shall then compare these formulas with M. Gauss's, and shall calculate the example he has chosen.

The difference of the two stars in right ascension, reduced by an angle proportionate to the diurnal motion during the interval of the observations, gives first an angle to the pole between two circles of declination passing through the points that were occupied by the stars at the moment of the observations: the two declinations and this angle being known, it it easy to resolve the triangle.

Let δ and δ' be the two declinations, and θ the angle; the formula ($\cot \delta' \cos \theta = \text{tang}\, x$) ....(1) gives the first segment of one side equal to $(90-\delta)$.

The second segment will then be $90-\delta-x=90-(\delta+x)$.

Suppose now that V is the angle to the first star, you will have $\text{tang}\, V = \dfrac{\text{tang}\, \theta .\ \sin .\, x}{\cos(\delta+x)}$ ......(2).

The arc D, which joins the two points observed, would be found by the formula $\cos D = \dfrac{\sin \delta' \sin(\delta+x)}{\cos x}$ .....(3): and the solution of this first triangle would require eleven different logarithms, but you may leave out one by making $\cot D = \cos V\, \text{tang}\, (\delta + x)$ ......(3*).

Thus this first triangle requires ten or eleven logarithms, according to the choice you may make. If in these formulas δ is changed into δ', and δ' into δ, you will have the angle V' to the second star, D being always the same distance.

Then the three sides of the triangle between the zenith and the two points observed will be known. Let W be the angle opposite to the second distance to the zenith, $\sin \frac{1}{2} W = \pm \left(\dfrac{\cos \frac{(h+h'+D)}{2} \sin \left(\frac{(h+h'+D)}{2} - h'\right)}{\cos h \sin D} \right)^{\frac{1}{2}}$ ......(4), h and h' being the two observed altitudes.

W may be either positive or negative. The circumstances will almost always indicate the choice that is to be made.

By changing h into h', and reciprocally, we would obtain the angle W' opposite to the first distance to the zenith. In all cases the solution may thus be obtained in two different manners, which prove each other mutually.

This

This analogy requires five logarithms, which being united to the ten or eleven preceding will make fifteen or sixteen at will.

Let $u = V \mp W$, u will then be the angle to the first star between the vertical and the circle of declination. In the same manner we should have $u = V + W'$ for the second star.

Knowing the angle u, you will endeavour to find the horary angle λ and the latitude φ by means of the following formulas, which are all similar to the formulas (1), (2) and (3).

Tang $z = \cos u \cot h$; tang $\lambda = \frac{\text{tang}\, u \sin z}{\cos(\delta + z)}$; tang $\varphi =$ $\cos\lambda$ tang $(\delta + z)$, (5), (6) and (7), by changing δ into δ', h into h', and λ into λ', and reciprocally, you will obtain the horary angle of the second star, by means of the second altitude.

Instead of the last formula, we might have $\sin\varphi = \frac{\sin h \sin(\delta + z)}{\cos z}$ ....(7*). This third triangle therefore requires ten or eleven more logarithms.

The entire solution thus requires five-and-twenty or seven-and-twenty logarithms, but the method requiring only five-and-twenty may always be preferred. Of these logarithms, none is used more than once.

These formulas are easy to establish, and require no effort of memory. We might obtain the angle W by the formula $\cos W = \frac{\sin h' - \sin h \cos D}{\cos h \sin D} = \tan h \cot D \left(\frac{\sin h'}{\sin h \cos D} - 1\right)$....(4*), but it would require seven logarithms instead of five: in the case $h = h'$ or of two equal heights, the formulas are on the contrary reduced to $\cos W = \frac{\sin h\,(1 - \cos D)}{\cos h \sin D} = \frac{\text{tang}\, h\; 2\sin^2 \frac{1}{2}D}{2 \sin \frac{1}{2}D \cos \frac{1}{2}D} = \text{tang}\, h\; \text{tang}\, \frac{1}{2}D$, which only requires three logarithms instead of five.

Cos W may belong to a negative as well as to a positive arc. It is this uncertainty which has already been remarked on the subject of $\sin^2 \frac{1}{2}W$. It is common to all methods.

It is not likely that a more easy or shorter solution can be found than the one contained in the foregoing formulas.

Let us now examine M. Gauss's, giving to the analytical calculations a more direct and elementary form.

M. Gauss first draws from spherical trigonometry the two fundamental equations;

$\text{Sin}\, h = \sin\delta \sin\varphi + \cos\delta \cos\varphi \cos\lambda$......(1).

$\text{Sin}\, h' = \sin\delta' \sin\varphi + \cos\delta' \cos\varphi \cos(\lambda - \theta)$......(2).

 He

He only applies two triangles, while we have made use of three, which have an intimate connexion and necessary relations. M. Gauss has thus proposed a much harder problem; but his analysis has set aside all difficulties in a very happy manner.

From the equation (1) it is easy to obtain $\cos^2 h = 1 - s^2 h = 1 - (\sin\delta \sin\phi + \cos\delta \cos\phi \cos\lambda)^2 = 1 - \sin^2\delta \sin^2\phi - \cos^2\delta \cos^2\phi \cos^2\lambda - 2\sin^2\delta \cos\delta \sin\phi \cos\phi \cos\lambda = 1 - s^2\delta + s^2\delta \cos^2\phi - \cos^2\delta \cos^2\phi \cos^2\lambda - 2\sin\delta \cos\delta \sin\phi \cos\phi \cos\lambda = \cos^2\delta + s^2\delta \cos^2\phi - \cos^2\delta \cos\phi + \cos\delta \cos^2\phi \sin^2\lambda - 2\sin\delta \cos\delta \sin\phi \cos\phi \cos\lambda = (\cos\phi\ s\lambda)^2 + (\cos\delta \sin\phi - \sin\delta \cos\phi \cos\lambda)^2$; and therefore dividing the whole by $\cos^2 h$ $1 = \left(\frac{\cos\phi \sin\lambda}{\cos h}\right)^2 + \left(\frac{\cos\delta \sin\phi - s\delta \cos\phi \cos\lambda}{\cos h}\right)^2 \ldots.(A)$.

Surely nothing can be easier than all those successive modifications which have led to the equation (A); but it is not easy to conceive why in the process $\sin^2\phi$, for example, has been left out, and $(1 - \cos^2\phi)$ substituted, instead of leaving out $\sin^2\delta$, and substituting $1 - \cos^2\delta$; and even there is no very conclusive reason why the value of $\cos^2 h$ should be sought for by means of that of $\sin h$, given directly by the problem; and in this generally consists the inconveniency of analytical demonstrations.

It is easy to perceive that $\left(\frac{\cos\phi \sin\lambda}{\cos h}\right)$ is the sine of the angle to the star in the triangle ZPA to the pole, the zenith and the first star. Thus sine ZA : sin ZP :-: sin P : sin $A = \frac{\sin PZ \sin P}{\sin ZA} = \frac{\cos\phi \sin\lambda}{\cos h}$.

The equation becomes, therefore, $1 = \sin^2 A + \left(\frac{\cos\delta \sin\phi - \sin\delta \cos\phi \cos\lambda}{\cos h}\right)^2 = \sin^2 A + \cos^2 A$; from which it is easy to conclude that $\cos A = \left(\frac{\cos\delta \sin\phi - \sin\delta \cos\phi \cos\lambda}{\cos h}\right)$ (A) will then be in the first triangle the angle to the star. M. Gauss called it u, without any other explanation, and makes $\sin u = \frac{\cos\phi \sin\lambda}{\cos h}$, which is the formula (3); $\cos u = \left(\frac{\cos\delta \sin\phi - \sin\delta \cos\delta \cos\lambda}{\cos h}\right)$, which is the formula (4.)

It might be concluded that $\tan u = \frac{\sin u}{\cos u} = \frac{\cos\delta \tan\phi - \sin\delta \cos\lambda}{\sin\lambda}$, but as we have already found the value

value of this angle, we may make use of it to obtain other useful equations. We have, to be sure, the analytical expression of the sine u and of the cosine u, but they contain the two unknown quantities φ and λ; they cannot, therefore, serve but as preparatives.

Previous to using them, let us draw from formula A general consequences which may be of service. Let A be the pole, B the zenith, and C the angle to the star; we shall have $\sin C = \frac{\sin AB \sin A}{\sin BC} = \left(\frac{\cos \varphi \sin \lambda}{\cos h}\right) \ldots\ldots B.$

$$\cos C = \left(\frac{\sin AC \cos AB - \cos AC \sin AB \cos A}{\sin BC}\right) \ldots\ldots C.$$

This last formula is general; it belongs to all spherical triangles, and expresses the relation between the three sides of any triangle whatever, and two of its angles. Generally, sine 1st side cosine 2d side—cosine 1st side sine 2d side cosine angle comprised = sine third side cosine angle opposed to 2d side .....(D): and $\frac{\text{sine 2d side into sine of the comprised angle}}{\text{sine third side}}$ = sin angle opposite to second side .....(E).

This formula, which we have just found by analysis, is the fourth of eighteen of the same kind I have found by synthesis, which I have explained in my Courses at the *College de France*: they are not of service in the common calculations of spherical triangles, because they require the knowledge of four of the parts of the triangles, instead of three, which are sufficient in all cases; but they are useful in analytical disquisitions, and we shall have occasion to use them again in the course of this memoir.

Supposing the angle to the first star or angle u is known, we shall conclude that $\sin \varphi = \sin h \sin \delta + \cos h \cos \delta \cos u \ldots\ldots (5)$; h and δ are the complements of the sides comprising the angle u, φ is the complement of the side opposite to angle u: here is then a mean to ascertain φ, as soon as we shall have determined the value of the third unknown quantity that has been introduced, that is to say, of angle u.

M. Gauss finds analytically the equation (5) by the combination of his formulas (1) and (3); but we endeavour to reduce all analytical operations to known rules of spherical geometry, in order to render the relation or rather the identity of the two methods more evident.

The equation (3) $\sin u = \frac{\cos \varphi \sin \lambda}{\cos h}$, by simply reversing the quantities, gives $\sin \lambda = \frac{\cos h \sin u}{\sin \varphi} =$

 sin

$\frac{\text{sin second side sin angle comprised}}{\text{sine third side}}$ (formula E): thus cosine λ = cosine of angle opposite to second side = $\frac{\text{sine 1st side cos 2d side} - \text{cos 1st side sin 2d side cos angle comprised}}{\text{sine 3d side}}$; according to the general formula (D).

Or else, $\cos \lambda = \frac{\cos \delta \sin h - \sin \delta \cos h \cos u}{\cos \varphi}$ .....(6).

Such in fact is the formula (6) of M. Gauss, who finds it, as well as formula (5), by combining the equations (1) and (3).

The formulas (3), (4), (5) and (6), contain, as we perceive, three unknown quantities instead of two; but they will soon serve to find the useful equations.

By developing the formula (2), we shall have $\sin h' = \sin \delta' \sin \varphi + \cos \theta \cos \delta' \cos \varphi \cos \lambda + \sin \theta \cos \delta' \cos \varphi \sin \lambda$.

Let us substitute in this formula the values of $\sin \varphi$ (equation 5), of $\cos \varphi \cos \lambda$ (equation 6), $\cos \varphi \sin \lambda$ (equation 3); we shall have $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \cos u \cos h \cos \delta \sin \delta' + \cos u \cos h \cos \theta \sin \delta \cos \delta' - \sin u \cos h \sin \theta \cos \delta = 0$......F.

Or, $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \cos u \cos h (\cos \delta \cos \delta' - \cos \theta \sin \delta \cos \delta') - \sin u \cos h (\sin \theta \cos \delta') = 0$.

Or, $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \cos h \sin \theta \cos \delta' \left(\frac{(\cos \delta \sin \delta' - \cos \theta \sin \delta \cos \delta')}{\sin \theta \cos \delta'} \cos u + \sin u\right) = 0$.

Suppose, for the sake of shortening,
$\frac{(\cos \delta \sin \delta' - \cos \theta \sin \delta \cos \delta')}{\sin \theta \cos \delta'} = \text{cotang V}$ ....(7).

The equation will be reduced to $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \cos h' \sin \theta \cos \delta' (\cot V \cos u + \sin u) = 0$.

Or, $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \cos h \sin \theta \cos \delta' \left(\frac{\cos V \cos u + \sin V \sin u}{\sin V}\right) = 0$.

Or, $\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta' - \frac{\cos h \sin \theta \cos \delta' \cos (V - u)}{\sin V} = 0$.

(V) is known by means of the equation (7), therefore the last equation contains only the unknown quantity $(V - u)$ or $(u - V)$, for it may be the one as well as the other: we shall

shall then have $\cos W = \cos (V - u) = \frac{\sin V}{\cos h \sin \theta \cos \delta'}$ $(\sin h' - \sin h \sin \delta \sin \delta' - \sin h \cos \theta \cos \delta \cos \delta') \ldots\ldots 8$: and $u = V - e\,(V - u)$, or $u = (u - V) + V$.

It is not easy to perceive, at first sight, how M. Gauss has been led to seek the auxiliary angle V, by means of the formula (7); but the processes of the calculations we have given, show how the author may have been led to it.

It remains to know whether the angle V might not be one of the angles of the two triangles we have made use of. I see first that the equation (7), which gives the value of it, may be put under a simpler form:

$$\text{Cot } V = \frac{\cos \delta \text{ tang } \delta'}{\sin \theta} - \sin \delta' \cot \theta.$$

$$\text{Or, tang } V = \frac{\sin \theta}{\cos \delta \text{ tang } \delta' - \cos \theta \sin \delta'}.$$

Therefore I now recognise the formula of spherical trigonometry, which gives the angle to the first star in the triangle of which $(90 - \delta)$ $(90 - \delta')$ are the sides, and θ the angle comprised; and which thus has one of its summits at the pole, and the other two at the points of the two stars.

The angle V is thus that which is determined by the two first analogies in the vulgar method.

The difference of the angles W and V, of the formulas 7 and 8, serves then to show the value of $W = \pm W \mp V$, or the angle to the first star in the triangle which has given rise to the equation (1).

In this triangle we know the sides $(90 - h)$ $(90 - \delta)$, and the angle comprised u: we shall get the angle to the pole of the first observation by the formula

$$\text{Tangent } \lambda = \frac{\cos h \sin u}{\cos \delta \sin h - \sin \delta \cos h \sin u} \ldots\ldots (9) \text{ which}$$

could be set down, $\text{tang. } \lambda = \frac{\sin u}{\cos \delta \text{ tang } h - \sin \delta \cos u}$.

λ is the horary angle of the first star at the time it has been observed; if it was then east, Æ first star $- \lambda =$ right ascension to the middle of the sky, from which you may conclude the time of the first observation, and the correction of the clock.

If the star was to westward, Æ first star $+ \lambda =$ right ascension to the middle of the sky, of which you will make the same use.

Lastly, the combination of equations (4) and (5) will give you $\text{tang } \varphi = \sin \lambda \left(\frac{\sin \delta \sin h + \cos \delta \cos h \cos u}{\cos h \sin u}\right)$ $\ldots\ldots (10)$.

Such,

Such, in fact, is M. Gauss's tenth formula.

It might be written, $\text{tang}\ \varphi = \frac{\sin\lambda}{\sin u}(\sin\delta\ \text{tang}\ h + \cos\delta$ $\cos u)$.

The trigonometrical method has given us above the simplest formula, $\sin\varphi = \frac{\sin h \sin(\delta+z)}{\cos z}$.

Or, $\sin\varphi = \sin\delta \sin h + \cos\delta \cos h \cos u$.

The problem is then completely resolved. It may be simplified; but let us previously make a useful remark, in order to distinguish, beween the two solutions of the problem, that which is applicable to the observations.

The triangle to the pole between the two spots where the stars have been remarked, gives $\sin V : \cos\delta' :: \sin\theta : \sin D$ = sin 3d side of the triangle.

The triangle to the zenith gives $\cos h' : \sin W :: \sin D : \sin(A - A')$ = sin of azimuth difference.

$$\text{Sin}\ (A - A') = \frac{\sin D \sin W}{\cos h} = \frac{\sin\varphi \cos\delta'}{\sin V} \cdot \frac{\sin W}{\cos h'}.$$

Or, $\sin W = \sin \frac{(A-A) \sin V \cos h'}{\sin\theta \cos\delta'}$; and $\frac{\cos h'}{\cos\delta'}$, is a quantity essentially positive.

Sin (A′−A) will always be positive, if you reckon the azimuths from one star to the other by the shortest distance.

Therefore sin W will have the same sign affixed to it as $\frac{\sin V}{\sin\theta}$; you will therefore know whether W is a positive or a negative angle.

Which is the equation given by M. Gauss in order to set all doubt aside:—there is no need of calculating it. It will also be easy without it to know the sign of $\left(\frac{\sin V}{\sin\theta}\right)$, which is sufficient.

If the two observations are both to eastward, $u = W - V$; if one is to eastward and the other to westward, $u = V - W$, unless (d Æ — motion of rotation) be negative.

In order to render the calculation of the angle V easier, M. Gauss makes (11) $\text{tang}\ F = \frac{\text{tang}\ \delta}{\cos\theta} = \frac{1}{\cos\theta \cot\delta'}$, or, $\cot F = \cos\theta \cot\delta'$; in which we perceive that his angle F is the complement of the subsidiary angle which I have named x.

$$(12)\quad \text{tang}\ V = \frac{\cos F\ \text{tang}\ \theta}{\sin(F-\delta)} = \frac{\text{tang}\ \theta \cos x}{\cos(\delta+x)}.$$

So for the angle V we may say that our formulas are identical.

(13) cos

(13) $\cos W = \frac{\cos V \text{ tang } h}{\text{tang } (F-\delta)} \left(\frac{\sin h' \sin F}{\sin h \sin \delta \cos (F-\delta)} - 1\right)$.

I make $\cos W = \text{tang } h \cos D \left(\frac{\sin h'}{\sin h \cos D} - 1\right) = \text{tang } h \cos V \text{ tang } (\delta + x) \left(\frac{\sin h \cos x}{\sin h \sin \delta' \sin (\delta' + x)} - 1\right)$.

Our equations are thus again identical, notwithstanding the apparent difference. I have sought for the arch D separately, but it is clear I could have done without: my calculation is somewhat the shortest.

If $h = h'$, as in the example chosen by M. Gauss, $\frac{\sin h'}{\sin h} = 1$, two logarithms are saved in each of the two methods: but the trigonometrical method shows a more considerable simplification.

My formula becomes $\cos W = \text{tang } h \cot D \left(\frac{1}{\cos D} - 1\right) = \frac{\text{tang } h \cot D}{\cos D} (1 - \cos D) = \frac{\text{tang } h \, 2 \sin^2 \frac{1}{2} D}{\sin D} = \frac{\text{tang} h . 2 \sin^2 \frac{1}{2} D}{2 \sin \frac{1}{2} D \cos \frac{1}{2} D} = \text{tang } h \tan \frac{1}{2} D$: a formula which the inspection of the triangle to the zenith, which in that case is isosceles, gives immediately.

I draw the same simplification from M. Gauss's formula; but it was not evident at first sight: his formula (8) then becomes $\cos W = \frac{\text{tang } h \sin V}{\sin \theta \cos \delta'} (1 - \sin \delta \sin \delta' - \cos \delta \cos \delta' \cos \theta) = \frac{\tan h \sin V}{\sin \theta \cos \delta'} (1 - \cos D)$.

By making $\sin \delta \sin \delta' + \cos \delta \cos \delta' \cos \theta = \cos D$. Thus $\cos W = \frac{\text{tang } h . \sin V \; 2 \sin^2 \frac{1}{2} D}{\sin \theta \cos \delta'} = \frac{\text{tang } h \; 2 \sin^2 \frac{1}{2} D}{\frac{\sin \theta \cos \delta'}{\sin V}} = \frac{\text{tang } h \; 2 \sin^2 \frac{1}{2} D}{\sin D} = \text{tang } h \text{ tang } \frac{1}{2} D$, as heretofore.

If we continue the comparison:

(14) $\cot G = \frac{\text{tang } h}{\cos u} = \frac{1}{\cos u . \cot h} = \cot z$, then my arc $z = 90 - G$.

(15) $\text{tang } \lambda = \frac{\cos G . \tan u}{\sin (G - \delta)} = \frac{\sin Z \text{ tang } u}{\cos (d + z)}$; our equations are identical.

(16) $\text{tang } \varphi = \cos \lambda \cos (G - \delta) = \cos \lambda \text{ tang } (\delta + z)$.

Thus

Thus the formulas (11), (12), (13), 14, (15) and (16), which contain the practical solution of the problem in M. Gauss's method, and to which he has attained by a very dexterous analysis, are the same as are taken at sight in the three triangles that have been resolved at all times by astronomers.

So that it is here demonstrated, *ipso facto*, that analysis can by its own strength lead to the same result as geometrical consideration. But it is not the less clear, that these last have the advantage of facility and conciseness; and we may add that it is always pleasing for the calculator to understand what he goes through; he sees that x and z are the first segments of the two bases, $90 - \delta - x$, and $90 - \delta - z$, the second segments of the same bases; W, V and u angles comprised between known sides. These notions would suffice for him to find all the formulas he wants, without having recourse to any book; while it is, as I may say, impossible to grave in our memory the six analytical formulas; so that we are obliged to follow them blindly, because we cannot divine what are the subsidiary arcs F, W, V, u and G, nor $(F - \delta)$ and $(G - \delta)$ in this problem.

From the demonstrated identity of the two methods, it is easy to conceive that in point of conciseness the difference cannot be great. In the angle W alone, to ascertain which the processes are not quite the same, the trigonometrical method is somewhat shorter, as will be seen by the calculated example.

M. Gauss lastly inquires by differentiation of fundamental formulas, what may be the influence of the errors dh and dh' of the two altitudes observed on the latitude and the horary angle λ. I have expressed this same effect by different formulas, with which I shall begin.

In the first place: It is clear that the errors dh and dh' can have no influence on the first three formulas which gave x, V and D, for those quantities only depend on the two stars.

The triangle to the zenith gives the equation $\cos W \cos h \sin D = \sin h' - \sin h \cos D$. Whence $dW \sin W \cos h \sin D - dh \sin h \cos W \sin D = dh' \cos h' - dh \cos h \cos D$. We shall suppose D constant $- dW = dh \left(\frac{\sin h \cos W \sin D - \cos h \cos D}{\sin W \cos h \sin D}\right) + \frac{dh' \cos h'}{\cos h \sin D \sin W} =$

$$\frac{dh' \cos h'}{\cot h \sin D \sin W} - \frac{dh \cos h \cos D}{\sin W \sin D \cos h} + dh \tang h \cos W$$

$$= - dh \left(\frac{\cos h \cos D - \sin h \sin D}{\cos h \sin D \sin W} \cos W\right) + \frac{dh' \cos h'}{\cos h \sin D \sin W}$$

$$+ dW$$

$+ dW = + dh\left(\frac{\cot D}{\sin W} - \text{tang } h \cot W\right) - \frac{dh' \cos h'}{\cos h \sin D \sin W}$, for we shall suppose D to be constant: thus $\pm dW = dh$, &c.....(1).

This equation may be put in another form: for,

Let $(90 - h)$ be the first side of the triangle, D the second, W the angle comprised; we shall have $- dW = - dh \left(\frac{\text{sin 1st side cos 2d side} - \text{cos 1st side sine 2d side cos contained } \angle}{\text{sin 1st side sin 2d side sine contained angle}}\right) + \frac{dh \text{ sin 3d side}}{\text{sin 1st side sine 2d side sin contained } \angle} = - dh \left(\frac{\text{sin 3d side cos angle opposite 2d side}}{\text{sin 1st side sin 2d side sin contained } \angle}\right) + \frac{dh'}{\text{sin 1st side sin } \angle \text{ opposite 2d side}}$
$= - dh \left(\frac{\cos \angle \text{ opposite 2d side}}{\text{sin 1st side sin } \angle \text{ op. 2d side}}\right) + \frac{dh'}{\text{sin 1st side sin } \angle \text{ oppos. 2d side}}$
$= - \frac{dh \cos (A - A')}{\cos h \sin (A - A')} + \frac{dh}{\cos h \sin (A - A')}$ A and A' being the azimuths in the two observations, $- dW = \frac{dh' - dh \cos (A - A')}{\cos h \sin (A - A')} = \frac{dh' - dh + 2\, dh \sin^2 \frac{1}{2} (A - A')}{\cos h \sin (A - A')}$; and $dW = \frac{dh \cos (A - A') - dh'}{\cos h \sin (A - A')} = \frac{dh \cot (A - A')}{\cos h} - \frac{dh'}{\cos h \sin (A - A')}$ .... 2.

$W - V = u$, then $dW = du$ when the two stars have been observed on the same side of the meridian; $d = V - W$, if they have been observed on different sides, then $du = - dW$.

In this second formula, it is supposed we are acquainted with A and A', or at least $(A - A')$.

But as $\cos h : \sin W :: \sin D : \sin (A - A') = \frac{\sin W \sin D}{\cos h}$; it must, besides, be known whether $(A - A')$ be more or less than $90°$, so that no great advantage is obtained by preferring the second formula to the first.

We have found above, $\sin \varphi = \sin h \sin \delta + \cos h \cos \delta \cos u - d\varphi = \frac{dh \cos h \sin \delta - dh \sin h \cos \delta \cos u}{\cos \varphi} - \frac{du \sin u \cos h \cos \delta}{\cos \varphi}$ ...(3); $dh = \left(\frac{\text{sin 1st side cos 2d side} - \text{cos 1st side sin 2d side cos } \angle \text{ comp.}}{\text{sin 3d side}}\right) - \frac{du \sin u \cos h \cos \delta}{\cos \varphi} = dh \frac{\text{sin 3d side cos angle opposite 2d side}}{\text{sin 3d side}} - du \cos h \sin A = dh \cos A - du \cos h \sin A\ (3^*) = dh \cos A - \frac{dh \cos (A - A') - dh'}{\cos h \sin (A - A')} \cos h \sin A = dh \cos A - \frac{(dh \cos (A - A') - dh') \sin A}{\sin (A - A')}$
$= \frac{dh \cos A \sin (A - A') - dh \sin A \cos (A - A') + dh' \sin A}{\sin (A - A')} =$

dh sin

$$\frac{dh \sin(A-A'-A) + dh' \sin A}{\sin(A-A')} = \frac{-dh \sin A' + dh' \sin A}{\sin(A-A')} \,..(3)$$

This is M. Gauss's formula, who computing the azimuths outwardly on the triangle, writes $(A'-A)$ which comes to the same.

This formula requires that A and A′ be known.

Now, $\cos\phi : \sin u :: \cos\delta : \sin A = \frac{\cos\delta \sin u}{\cos\phi}$; Or, $\cos A = \frac{\sin\delta - \sin h \sin\phi}{\cos h \cos\phi}$. The second value is longer to compute, but it leaves no doubt on the sort of the angle A which will be obtuse. If $\sin\delta \angle \sin h \sin\phi$; we shall likewise have $\cos A = \frac{\sin\delta' - \sin h' \sin\phi}{\cos h' \cos\phi}$.

After having calculated A and A′, we shall have $(A-A')$, and we may determine $d\phi$ without going through du.

We have, lastly; $\sin u \cot\lambda = \tang h \cos\delta - \sin\delta \cos u - du \cos u \cot\lambda - \frac{d\lambda \sin u}{s^2\lambda} = \frac{dh \cos\delta}{\cos^2 h} + du \sin u \sin\delta$,

$$du \cos u \operatorname{cotang}\lambda - \frac{dh\cos\delta}{\cos^2 h} - du \sin u \sin\delta = \frac{d\lambda \sin u}{\sin^2\lambda}$$

$$d\lambda = \frac{du \cot u \cos\lambda \sin\lambda}{\sin u} - \frac{dh \cos\delta \sin^2\lambda}{\cos^2 h \sin u} - \frac{du \sin\delta \sin^2\lambda}{1} =$$

$$du \sin\lambda(\cos u \cos\lambda - \sin\delta\sin\lambda) - \frac{dh \cos\delta \sin\lambda \sin\lambda}{\cos h \cos h \cos u} \,..(5)$$

$$= du\sin\lambda\left(\frac{\cos u \cos\lambda - \sin\delta \sin\lambda \sin u}{\sin u}\right) - \frac{dh \sin A \sin\lambda}{\cos h \sin u} =$$

$$-\frac{du \sin\lambda}{\sin u}(\sin\lambda \sin u \sin\delta - \cos\lambda \cos u) - \frac{dh \sin A}{\cos\phi} =$$

$$-\frac{du \cos h}{\cos\phi}(\cos A) - \frac{dh \sin A}{\cos\phi} \ldots (5^*) = -\frac{\cos h \cos A}{\cos\phi}$$

$$\left(\frac{dh \cos(A-A') - dh'}{\cos h \sin(A-A')}\right) - \frac{dh \sin A}{\cos\phi} = -\frac{dh \cos A \cos(A-A') - dh \cos A}{\cos\phi \sin(A-A')}$$

$$-\frac{dh \sin A}{\cos\phi} = -\frac{dh \cos A \cos(A-A')}{\cos\phi \sin(A-A')} + \frac{dh \cos A}{\cos\phi \sin(A-A')} -$$

$$\frac{dh \sin A \sin(A-A')}{\cos\phi \sin(A-A')} = -dh\left(\frac{\cos A \cos(A-A) + S, A \sin(A-A)}{\cos\phi \sin(A-A')}\right) \frac{-dh' c. A}{}$$

$$= -\frac{dh \cos(A - A + A') - dh' \cos A}{\cos\phi \sin(A-A')} = \frac{-dh \cos A' + dh' \cos A}{\cos\phi \sin(A-A')} \,..(6).$$

If we take the A's outwardly on the triangle, the formula will be the same as M. Gauss's, and will become $d\lambda = \frac{+dh \cos A' - dh' \cos A}{\cos\phi \sin(A-A')}$.

Thus my formulas 1, 3, 5, can be brought back to M. Gauss's 4 and 6, who has found them by other means.

The

The formulas 4 and 6 having for denominator the sine of $(A-A')$ M. Gauss concludes that if $A = A'$; that is to say, if the two stars have been observed in the same vertical, or in different verticals of 180°, the errors dh dh' will have a prodigious influence.

The same consequence may be drawn from my first formula, for it bears for its denominator the sine W, and in the same case $W = 180$, and $\sin W = 0$ like sine $(A - A')$.

This consequence, however, is subject to some remarkable exceptions

For, suppose that $A = A'$, that is to say, the two stars have been observed in the same vertical line, a single altitude will suffice, for $h - h' = D$, $u = 180 - V$ &c. $- u + V = 180° = W$, u will then be known without error, the formula (3*) $d\phi = dh \cos A - du \cos h \sin A$ is reduced thereby to the term $dh \cos A$.

[To be continued.]

XVIII. *Observations upon Luminous Animals.* By JAMES MACARTNEY, *Esq.*

[Concluded from p. 35.]

THE remarkable property of emitting light during life is only met with amongst animals of the four last classes of modern naturalists, viz. mollusca, insects, worms, and zoophytes.

The mollusca and worms contain each but a single luminous species; the *pholas dactylus* in the one, and the *nereis noctiluca* in the other.

Some species yield light, in the eight following genera of insects: *elater, lampyris. fulgora, pausus, scolopendra, cancer, lynceus**, and *limulus*. The luminous species of the genera lampyris and fulgora are more numerous than is generally supposed, if we may judge from the appearance of luminous organs to be seen in dried specimens.

Amongst zoophytes we find that the genera *medusa, beroe*†, and *pennatula*, contain species which afford light.

The only animals which appear to possess a distinct organization

* The animal discovered by Riville off the coast of Malabar in 1754 is certainly a testaceous insect, and appears to belong to the genus lynceus of Müller.

† The luminous zoophyte for which Peron has lately instituted the new genus pyrosoma, appears to me to be a beroe, and only worthy of a specific distinction.

ganization for the production of light, are the luminous species of lampyris, elater, fulgora, and pausus.

The light of the lampyrides is known to proceed from some of the last rings of the abdomen, which when not illuminated are of a pale yellow colour. Upon the internal surface of these rings there is spread a layer of a peculiar soft yellow substance, which has been compared to paste, but by examination with a lens I found it to be organized like the common interstitial substance of the insect's body, except that it is of a closer texture, and a paler yellow colour. This substance does not entirely cover the inner surface of the rings, being more or less deficient along their edges, where it presents an irregular waving outline. I have observed in the glow-worm, that it is absorbed, and its place supplied by a common interstitial substance, after the season for giving light is past.

The segments of the abdomen, behind which this peculiar substance is situated, are thin and transparent, in order to expose the internal illumination.

The number of luminous rings varies in different species of lampyris, and as it would seem at different periods in the same individual.

Besides the luminous substance above described, I have discovered in the common glow-worm, on the inner side of the last abdominal ring, two bodies, which to the naked eye appear more minute than the head of the smallest pin. They are lodged in two slight depressions, formed in the shell of the ring, which is at these points particularly transparent. On examining these bodies under the microscope, I found that they were sacs containing a soft yellow substance, of a more close and homogeneous texture than that which lines the inner surface of the rings. The membrane forming the sacs appeared to be of two layers, each of which is composed by a transparent silvery fibre, in the same manner as the internal membrane of the respiratory tubes of insects, except that in this case the fibre passes in a spiral instead of a circular direction. This membrane, although so delicately constructed, is so elastic as to preserve its form after the sac is ruptured and the contents discharged.

The light that proceeds from these sacs is less under the control of the insect than that of the luminous substance spread on the rings: it is rarely ever entirely extinguished in the season that the glow-worm gives light, even during the day; and when all the other rings are dark, these sacs often shine brightly.

The

The circumstance of there being points which give a more permanent light than the other parts of the luminous rings of the abdomen, has been noticed before by the Comte G. de Razoumouski. He states the number of these luminous points to vary from two to five*.

I must however remark, that I never saw more than two of these luminous points, which were always upon the last ring of the body, and that the figures which accompany the memoir of the Comte de Razoumouski bear scarcely any resemblance to the insect they are intended to represent; from which we may fairly suspect him of inaccuracy in other particulars.

As far as my observation has extended, the small sacs of luminous substances are not found in any species of lampyris, except the glow-worm of this country. Thunberg mentions that the lampyris japonica has two vesicles on the tail, which afford light.

The organs for the production of light in the genus elater are situated in the corcelet; these likewise consist of a peculiar yellow substance, placed behind transparent parts of the shell, which suffer the natural colour of this substance to be seen through them in the day, and when illuminated give passage to the light.

On dissecting the organs of light in the elater noctilucus, I found that there is a soft yellow substance, of an oval figure, lodged in the concavity of the yellow spots of the corcelet, which parts are particularly thin and transparent in this species. This substance is so remarkably close in its structure, that at first view it appears like an inorganic mass, but with a lens it is readily perceived to be composed of a great number of very minute parts or lobules closely pressed together. Around these oval masses, the interstitial substance of the corcelet is arranged in a radiated manner, and the portion of the shell that immediately covers the irradiated substance is in a certain degree transparent, but less so than that which lies over the oval masses: it is therefore probable, that the interstitial substance in this situation may be endowed with the property of shining. A fasciculus of the muscles of the corcelet arises in the interior of the oval masses of the luminous substance, but not apparently with any design, as it contributes, with the adjacent fasciculi, to move the anterior feet.

In the elater ignitus, the masses of luminous substance are extremely irregular in their figure: they are situated

* *Mem. de la Soc. de Lausanne*, tome ii.

nearly

nearly at the posterior angles of the corcelet, and are more loose in their texture than the oval masses of the noctilucus, resembling rather in composition the interstitial substance which surrounds these masses in that species. The shell of the corcelet is somewhat thinner, and more transparent along both sides of the margin, than at other places; but it is not, as in the noctilucus, elevated, and peculiarly clear and thin immediately over the seat of the luminous organ: consequently, the light emitted by the elater ignitus cannot be very brilliant.

I have not been able to procure any specimen of the elater phosphorea, but from the accounts of naturalists it appears to resemble in every respect the elater noctilucus; indeed I have great doubts of the phosphorea being a distinct species.

I have had an opportunity of examining, preserved in a moist way, two species of fulgora, the candelaria and laternaria. The light in this genus has been observed to issue from the remarkable proboscis on the fore part of the head. This part has always been described by authors as hollow or empty, which I have found to be perfectly correct; and what is more extraordinary, that the cavity communicates freely with the external air, by means of a chink or narrow aperture, placed on each side of the root of the proboscis. This projection is covered internally by a membrane, between which and the horny part or shell there appears to be interposed a pale reddish coloured soft substance, that is arranged in the candelaria in broad lines or stripes; but it is so thin, that I could not distinctly examine its structure, or absolutely determine, whether it should be considered as a substance intended to furnish the light of these insects, or the pigment upon which the colour of the proboscis depends.

The globes of the antennæ constitute the organs of light in the pausus spherocerus. Dr. Afzelius, who discovered the luminous property in this species, compares them to lanterns spreading a dim phosphoric light*. The rarity of the insect put it out of my power to examine its structure; but from the form and situation of its organs of light, it is most probable they are constructed like those of the fulgoræ.

It has been conjectured by Carradori and others, that the lampyrides were enabled to moderate or extinguish their light, by retracting the luminous substance under a mem-

* Linn. Trans. vol. iv.

brane;

brane; but neither in them, or any of the other luminous insects, have I found an apparatus of this sort. The substance furnishing the light is uniformly applied to corresponding transparent parts of the shell of the insect from whence it is not moved; indeed a membrane, if it did exist, would have but little effect in obscuring the light, and never could serve to extinguish it. The regulation of the kind and degree of the luminous appearance does not depend upon any visible mechanism, but, like the production of the light itself, is accomplished by some inscrutable change in the luminous matter, which in some animals is a simple operation of organic life, and in others is subject to the will.

It is worthy of remark, that in all the dissections I have made of luminous insects, I did not find that the organs of light were better or differently supplied with either nerves or air tubes, than the other parts of the body. The power of emitting light likewise exists in many creatures which want nerves, a circumstance strongly marking a difference between animal light and animal electricity.

With the exception of the animals above mentioned, the exhibition of light depends upon the presence of a fluid matter.

In the pholas dactylus, the luminous fluid is particularly evident, and in vast quantity; it is recorded by Pliny, that this fluid is like liquid phosphorus, and renders every object luminous with which it comes into contact. Reaumur also found that it was diffusible in water, or any other fluid in which the animal might be immersed *.

The shining of the scolopendra electrica I have always observed to be accompanied by the appearance of an effusion of a luminous fluid upon the surface of the animal, more particularly about the head, which may be received upon the hand, or other bodies brought into contact with the insect at the moment, and these exhibit a phosphoric light for a few seconds afterwards. This fluid, however, I never could discover in the form of moisture, even upon the clearest glass, although examined immediately with the most scrupulous attention by a lens: it must therefore be extremely attenuated.

The same appearance has been observed during the illumination of the nereis noctiluca by Fougeroux de Bondaroy †.

The animal discovered by Riville shed a blue liquor, which illuminated the water for a distance of two or three lines ‡.

* *Mem. de l'Acad. des Sc.* 1712. † Ibid. 1767.
‡ *Mem. Etrang. de l'Acad. des Sc.* tome iii.

Spallanzani relates, that the medusa which he examined communicated the property of shining to water, milk, and other fluids, on being rubbed or squeezed in them *.

The luminous fluid is in some instances confined to particular parts of the body, and in others is diffused throughout the whole substance of the animal.

In the scolopendra electrica, it appears to reside immediately under the integuments. In the lynceus discovered by Riville, it is contained in the ovary. If I may judge from my own observations, every part of the body of the medusæ is furnished with this fluid, as there is no part I have not seen illuminated under different circumstances; but Spallanzani affirms that it is only found in the large tentacula, the edges of the umbella, and the purse or central mass; which he proved, he says, by detaching these parts successively, when they shone vividly, while the rest of the body neither gave light or communicated any luminous appearance to water †.

Spallanzani discovered a mucous luminous fluid in the plumule of the pennatula phosphorea ‡.

The phænomenon of animal light has been attempted to be explained in different ways. By many persons it was formerly ascribed to a putrefactive process; but since the modern theories of combustion became known, it has been generally believed to depend upon an actual inflammation of the luminous substance, similar to the slow combustion of phosphorus. Others have accounted for the luminous effect, by supposing the matter of light to be accumulated, and rendered latent under particular circumstances, and afterwards evolved in a sensible form.

The opinion of the light of living animals being the consequence of putrefaction, is evidently absurd, and contradictory to all observation on the subject. It has been proved by the experiments of Dr. Hulme and others, that even the luminous appearances of dead animals are exhibited only during the first stages of the dissolution of the body, and that no light is emitted after putrefaction has really commenced.

Spallanzani, who was the most strenuous advocate for the phosphorescent nature of animal light, stated that glow-worms shone more brilliantly when put into oxygen gas; that their light gradually disappeared in hydrogen or in azotic gas, and was instantly extinguished in fixed air; that

* Spallanzani's Travels in the Two Sicilies, vol. iv.

† *Memoria sopra le meduse fosforiche, Mem. della Soc. Ital.* tomo vii.

‡ *Mem. della Soc. Ital.* tomo ii.

it was also lost by cold, and revived by the application of a warm temperature. He conjectured that the luminous matter of these insects was composed of hydrogen and carbonated hydrogen gas.

Forster relates, in the Lichtenberg Magazine for 1783, that on putting a lampyris splendidula into oxygen gas, it gave as much light as four of the same species in common air.

Carradori has made some experiments upon the lucciole, (lampyris italica) which led him to deny its phosphorescence. He found that the luminous portion of the belly of the insect shone in vacuum, in oil, in water, and different liquids? and under different circumstances, where it was excluded from all communication with oxygen gas. He accounts for the result of Forster's experiment, by supposing that the worm shone more vividly, because it was more animated in oxygen gas than in common air.

Carradori adopts on this subject the doctrine of Brugnatelli, and ascribes the luminous appearances of animals to the condensation and extrication of light in particular organs, which had previously existed in combination with the substance of their bodies. He supposes the light to be originally derived from the food, or the atmospheric air taken into the body; in short, that certain animals have the peculiar property of gradually imbibing light from foreign bodies, and of afterwards secreting it in a sensible form *.

The following experiments which I made upon this subject, would lead me to make different conclusions than those of the preceding authors.

Experiment 1.—A glow-worm was put into a glass of water, in which it lived nearly two hours, and continued to emit light as usual, until it died, when the luminous appearance entirely ceased.

Experiment 2.—The luminous substance was extracted from the beforementioned glow-worm, and from others killed in different ways, but it afforded no light.

Experiment 3.—The sacs containing the luminous matter were cut from the bellies of *living* glow-worms, and shone uninterruptedly for several hours in the atmosphere, and after their light became extinct, it was revived by being moistened with water; some of these were put into water in the first instance, in which they continued to shine unremittingly for 48 hours.

* *Annal di Chimica*, tomo xiii. 1797.

Experiment 4.—The luminous substance of a glow-worm was exposed to a degree of heat which would have been sufficient to inflame phosphorus, without increasing the brilliancy of its light; and further, it could not be made to burn by being applied to a red hot iron, or to the flame of a candle.

Experiment 5.—A delicate thermometer was introduced amongst some living glow-worms, during the time they gave out much light: the temperature of the room being 69, the instrument rose to 75, 76, and 77, according to circumstances, as the warmth was reflected from the hand, or dissipated by the worm crawling over cold substances. The luminous portion of the tail, when very brilliant, appeared to raise the thermometer more quickly than the other parts of the body, but it was not invariably the case. When shining strongly, I thought that the luminous rings communicated the sensation of warmth to the hand; but this was probably a deception, as the actual degree of heat was not sufficient for such an effect. It should however be mentioned, that in Templar's observations on the glow-worm, he said his feelings deceived him, if he did not experience some heat from the shining of the insect *.

Experiment 6.—To satisfy myself how far the evolution of heat during the shining of glow-worms depended upon the life of the animals, I cut off the luminous portion of the tail from several living worms, and I found that if the thermometer was applied to them immediately, it was raised by them one or two degrees; but after these parts were dead, although they continued to emit light, they produced no effect whatever upon the instrument.

Experiment 7.—Some hemispherical medusæ were put into a spoon containing a small quantity of sea water, and held over a burning candle. As soon as the water became heated the medusæ appeared like illuminated wheels, the spots at the margin and centre alone emitting light; in which manner they shone vividly and permanently for about 20 seconds, when they shrunk and died, after which they were no longer luminous.

Experiment 8.—Some of the same species were put into spirits: a strong and unremitting light was instantly given out, which issued from the central and marginal parts, as in the preceding experiment, and continued until they died.

Experiment 9.—Some of the scintillating and hemispherical species of medusa, contained in a small glass jar, were

* Phil. Trans. No. 72.

introduced

introduced into the receiver of an air-pump, and the air being exhausted, they shone as usual when shaken; if any difference could be perceived, the light was more easily excited, and continued longer in vacuum.

I wished next to try the influence of electricity on the luminous property of animals.

Experiment 10.—A medusa hemispherica was placed in a small glass dish, containing a quantity of water merely sufficient to allow the animal to preserve its figure; being insulated, it was electrified, and sparks drawn from it, which had not the slightest effect; the experiment was repeated several times with different individuals, but without exciting the animals to throw out light.

Experiment 11.—Some hemispherical medusæ were placed in contact with the two ends of an interrupted chain, and slight electric shocks passed through them. During the very moment of their receiving the shock no light was visible, but immediately afterwards the medusæ shone like illuminated wheels, which appearance remained for some seconds. Upon the closest inspection with a magnifying glass, no contractile motion could be perceived to accompany the exhibition of the light. The application of electricity in this instance seems to have acted merely as a strong mechanic shock.

The above experiments on the luminous medusæ were made at Herne, with the assistance of George May, Esq. of Stroud-house, and in the presence of a large company, capable of accurately distinguishing their results.

It seems proved by the foregoing experiments, that so far from the luminous substance being of a phosphorescent nature, it sometimes shows the strongest and most constant light, when excluded from oxygen gas; that it in no circumstances undergoes any process like combustion, but is actually incapable of being inflamed; that the increase of heat, during the shining of glow-worms, is an accompaniment, and not an effect of the phænomenon, and depends upon the excited state of the insect; and lastly, that heat and electricity increase the exhibition of light, merely by operating like other stimuli upon the vital properties of the animal.

In confirmation of these opinions, I may quote the high authority of the Secretary of this Society, who has found that the light of the glow-worm is not rendered more brilliant in oxygen, or in oxygenated muriatic gas, than in common air; and that it is not sensibly diminished in hydrogen gas.

 I may

I may further add, that Spallanzani's experiments of diffusing the luminous liquor of the medusa in water, milk, and other fluids, are in direct contradiction of his own theory, as is also the extinction of the light of these mixtures by the application of a high degree of heat.

If the light emitted by animals were derived from their food, or the air they respire, as supposed by Carradori, the phænomenon should be increased or diminished, according to the quantity of food or air that the creatures consume; but we do not find this to be the case; for in those situations where they are sometimes found to be most luminous, they are deprived, in a great measure, of these assumed sources of their light.

In fact, the luminous exhibitions of living animals are not only independent of all foreign light, but are frequently destroyed by the latter. I have always found the shining of the medusæ to cease upon the rising of the moon, or at the approach of day; and when out of the sea, I never could excite them to throw out light until they had been kept for some time in the dark; all the luminous insects likewise secrete themselves as much as possible during the day time, and go abroad only at night. I have, it is true, found that the scolopendra electrica will not shine unless it has been previously exposed to solar light; but I have observed that it shone as brilliantly and as frequently, after being kept a short time in a light situation, as when left uncovered the whole day. The circumstance of the scolopendra requiring exposure previous to its giving out light, is very unaccountable, as the insect, when left to itself, always seeks as much as possible concealment during the day; indeed it is the opinion of some naturalists that it is killed by the light of the sun.

The opinions of Brugnatelli and Carradori are connected with some general doctrines, respecting the nature of light, which I shall not at present venture to discuss. It appears to me, that the question is still unresolved, whether light has a substantial existence, or is a phænomenon depending upon certain operations or conditions of the ordinary forms of matter. But the highly ingenious researches of Count Rumford, on the laws of what have been called subtile fluids, and the extraordinary advances lately made by Mr. Davy, on the decomposition of substances that were hitherto looked upon as elementary, give us reason to hope, that future investigations may unfold views of the material world, of which we can at present have only an indistinct conception; that new modes of analysis may enable us to see

things,

things, not "through a glass darkly," but more nearly as they are; and that the boundaries of physical and metaphysical science, now so far asunder, may be made to approach each other.

In the present state of our knowledge, our business should be, to collect, arrange, and compare phænomena, rather than to speculate upon their nature. Nevertheless, I cannot refrain from observing, that the circumstances attending the luminous appearance of living animals, are much more favourable to the supposition of light being a property than a substance. The quantity of light emitted by an animal in a certain time, (admitting it to be matter) far exceeds that which could be possibly supplied by the sources from whence it is usually supposed to be derived. Thus the luminous appearance of some medusæ may be continued with the intermission of short intervals for an indefinite time, notwithstanding the creature be kept in darkness, and without any other food than what a small quantity of filtered sea-water would afford. The uninterrupted and long continued light that is sometimes evolved by the luminous sacs, and the ova of the glow-worm, is also inconsistent with the notion of an accumulation and subsequent dispersion of a material substance.

I shall terminate this paper by an enumeration of the several conclusions, that are the result of the observations I have been able to make upon the phænomena of animal light.

The property of emitting light is confined to animals of the simplest organization, the greater number of which are inhabitants of the sea.—The luminous property is not constant, but in general exists only at certain periods, and in particular states of the animal's body.—The power of showing light resides in a peculiar substance or fluid, which is sometimes situated in a particular organ, and at others diffused throughout the animal's body.—The light is differently regulated, when the luminous matter exists in the living body, and when it is abstracted from it. In the first case, it is intermitting, or alternated with periods of darkness; is commonly produced or increased by a muscular effort; and is sometimes absolutely dependent upon the will of the animal. In the second case, the luminous appearance is usually permanent until it becomes extinct, after which it may be restored directly by friction, concussion, and the application of warmth; which last causes operate on the luminous matter (while in the living body,) only indirectly, by exciting the animal.—The luminous matter, in all situ-

 ations,

ations, so far from possessing phosphoric properties, is incombustible, and loses the quality of emitting light, by being dried, or much heated.—The exhibition of light, however long it may be continued, causes no diminution of the bulk of the luminous matter. It does not require the presence of pure air, and is not extinguished by other gases.

The luminous appearance of living animals is not exhausted by long continuance, or frequent repetitions, nor accumulated by exposure to natural light; it is therefore not dependent upon any foreign source, but inheres as a property, in a peculiarly organized animal substance or fluid, and is regulated by the same laws which govern all the other functions of living beings.

The light of the sea is always produced by living animals, and most frequently by the presence of the medusa scintillans. When great numbers of this species approach the surface, they sometimes coalesce together, and cause that snowy or milky appearance of the sea, which is so alarming to navigators. These animals, when congregated on the surface of the water, can produce a flash of light, somewhat like an electric coruscation. When the luminous medusæ are very numerous, as frequently happens in confined bays, they form a considerable portion of the mass of the sea, at which times they render the water heavier, and more nauseous to the taste; it is therefore adviseable to always strain sea-water before it is drunk.

The luminous property does not appear to have any connection with the œconomy of the animals that possess it, excepting in the flying insects, which by that means discover each other at night, for the purpose of sexual congress.

Explanation of the Figures (*Plates* I and II.)

Fig. 1. The cancer fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural size.

Fig. 2. The same animal magnified.

Fig. 3. The medusa pellucens, also found by Sir Joseph Banks, represented of the natural magnitude.

Fig. 4. The limulus noctilucus, discovered by Captain Horsburgh, considerably enlarged.

Fig. 5. The luminous medusa, discovered by me, which I conceive to be the medusa hemispherica: it is shown of the largest size I met with.

Fig. 6. The central process of this animal's body magnified, in order to explain its structure. The thick tentacula in

in which it terminates are seen covered with small cups or suckers.

Fig. 7. The beroe fulgens, discovered by me, shown in its most elongated or relaxed form, which it assumes commonly when swimming quickly.

Fig. 8. The same animal in the most contracted form.

Fig. 9. The minute species of medusa, discovered by me, which is the most frequent cause of the luminous appearance of the sea, represented of the natural size.

Fig. 10. The same animal magnified, exhibiting a puckered or tucked-in appearance on one side.

Fig. 11. Is the animalcule discovered by Forster, of the natural size.

Fig. 12. The same, greatly magnified, to show the intestinal parts. Both these figures are copied from the original drawings, in the possession of the Right Hon. Sir Joseph Banks.

Fig. 13. Is an enlarged view of the inferior surface of the abdomen of the lampyris lucida, after the integument had been removed. *a a a* represent the three masses of luminous substance which are applied to the three last rings of the abdomen. *b b b* the arrangement of cellular or interstitial substance on the other abdominal rings, which gives the pale colour to the whole belly of this insect.

Fig. 14. Represents the common glow-worm, with the posterior portion of the back cut away to expose the sacs of luminous matter in situ on the last ring of the belly. *a* indicates the sac of one side; the intestine is seen to lie between them.

Fig. 15 and 16. Are the sacs of the glow-worm prodigiously magnified to show their structure. Fig. 16 is cut open to expose the luminous matter it contains: the coat of the sac is still seen to preserve its figure.

Fig. 17. Is the elater noctilucus, with the shell of the corcelet removed on one side, by which the organ of light is uncovered. *a* the yellow transparent spot of the corcelet. *b* the oval mass of luminous substance surrounded by an irradiation of the interstitial substance. *c* the ends of the muscles which were on the inside of the corcelet.

Fig. 18. Is the posterior angle of the corcelet of the elater noctilucus magnified. *a* the radiated appearance which the interstitial substance has round the oval mass of luminous matter. This mass is seen to consist of a number of smaller parts. *b* shows the appearance of the interstitial substance, where it passes down between the muscles. *c* the ends of the muscles of the back. *d* the shell of the corcelet.

Fig.

Fig. 19. Represents the elater ignitus. *a* is the mass of luminous substance of one side, seen indistinctly through the back part of the semitransparent portion of the corcelet. *b* is the luminous mass of the other side, exposed by removing a part of the shell of the corcelet.

XIX. *A List of about* 280 *Mines of Lead,—some with Zinc, Manganese, Copper, Iron, Fluor, Barytes, &c. in and near to Derbyshire. By Mr.* JOHN FAREY, Sen., *Mineralogical Surveyor.*

SIR, I INCLOSE a list of such mineral Veins or Mines as have been visited or information obtained concerning, in the course of the mineral Survey in which I have been engaged, since the year 1807: I do not offer it as a complete list of the mines, but as a selection from the most productive and important ones, or those which are calculated to illustrate some point of interest, in the progress of mining, or in a geological view. As I observed respecting the List of *Collieries* (in your 35th volume, page 432), many of these *Mines* were long ago discontinued, but as in most instances, further quantities of ore lay in the deep, below level or beneath the toadstone strata, the recording of such, may not be without its uses. In the manuscript of the first volume of my Report, which is now printing by order of the Board of Agriculture, I have given an alphabetical list of these mines, with the most important or remarkable products, and particulars respecting each; which mines all produce blue lead ore or galena, I believe, and the greater part of them in rake-veins in the limestone rocks; and I there distinguish, the other variable and more rare particulars, such as pipe-veins, flat-works, and in which of the limestone rocks or toadstones, &c. they occur; galena being found in toadstone between the limestones, or in the shale above them; white or green ores of lead, or silver combined therewith in notable quantities: copper ore, calamine, black-jack, black-wad, pyrites (iron), ochres; fluor spars, blue-john, barytes, calcareous crystals; sulphur, bitumen, petroleum; china-clay, steatite: cherts, toadstones, or clay-wayboards in the strata or veins: crooked, crossing, haded or squinted rakes; caverns, slickensides, faults, gravel or extraneous fossils in the veins, &c.

Without doubt, some mines that present instances, and perhaps, striking ones, of the particular phænomena mentioned above, or perhaps other curious ones, have escaped me

me in prosecuting my Survey, or in compiling these lists; in which case, I shall be thankful to any of your readers who may happen to possess or discover any such, if they will freely communicate them, as well as their remarks on the above, in order that as complete and useful lists and particulars as possible, may at length be given to the public, in my intended Mineral History and Map of this highly interesting district; which last will show, the great mining tract of limestone strata in Derbyshire and Staffordshire, to be extended over 105,000 acres, without including the shale and other occasional depositories of metallic ores in the district.

I am, sir,
Your obedient servant,
JOHN FAREY, Sen.

12, Upper Crown street, Westminster,
Feb. 1, 1811.

Places' Names.	Names of Mines.
Alport, in Yolgrave	Abbotshole,—Blithe,—and Wheels-Rake.
Alveton, E. of Cheadale, in Staffords.	Wire-Mill.
Ashford, near Bakewell	Glade-Rake,—and Greensward-Rake.
Ashover	Cockwell,—Fall-hill,—Town-head,—and Westedge.
Autherley, near Macclesfield, Cheshire	Autherley-Edge.
Bakewell	Birds-head, — Mockshaw, — Red-Rake,—and Warm-bath.
Birchwood-Park, in Roston	Birchwood-Park.
Bolterstone Chapel, NW of Sheffield, Yorkshire	Broomhead-Mill,—and Wig-twizle.
Bonsal	Ball eye,—Blakelow,—Bonsal-Leys,—Fiery-dragon.—Gorsey-Dale,—Hang-worm,— Porters',—Salters'-way, — Slack, — Stubben, — and White-low.
Bradburne	Mouldridge (near Pike-Hall).
Bradwell, in Hope	Hell-Rake, — Moss-Rake, — Mule-spinner, — Picture-end, — Raddle-pits,—Small-dale-head,—Tanners'-venture (at Hazlebadge),— Virgin, and Wet-rake.

Bras-

Places' Names.	Names of Mines.
Brassington, near Bradburne	Bald-mare,— Cursed-moor,—Green-linnet, — Meers, — Providence, — Suckstone, — Upper-field, — and White Mine.
Brushfield, near Taddington	Booth-Lee, — Lucky ploughman, — Putty-hill,—and Well-close.
Caldon, in Staffords.	Ribden.
Calke	Dimsdale.
Calver, in Bakewell	Dog-Rake,—and Peak-pipe.
Carsington	Carsington-Hill,— Nursery-end, — and Perseverance.
Castleton	Cliff-side,—Coal-pit-hole,—Dirtlow, —Fore-side,—Millers pipe,—New-Rake,— Nunleys,— Odin, — Old-Tor, — Redseats, — Speedwell (or Lanehead), — Water-hull-pipe,— Weather-Rake,—and Wet-Rake.
Chelmerton	Daily-bread.
Crich	Bacchus - pipe, — Barkers - field, — Caulk,— Church - Rake, — Crich-Cliff, — Crooked - Rake, — Hard-Rake, — Hazlehurst, — Pearson's-venture, — Raven Tor, — Rowbottom,—and Wards.
Cromford, near Wirksworth	Adventure,—Ash-cross,—Barrow,— Carrion-hole, – Cawk, – Dovestone-Leys,—Gang,– Goodluck, – Green-way-field,— Hading-vein,— Meers Tinley,—and Venture.
Elton, near Winster	Coast - Rake, — Cow-Close, — Old-Isaac's-venture,—Portaway-pipe, – Rath-Rake,—and Smiling-fancy.
Eyam............	Black-hole, — Broad-low, — Brook-head,–Broomhead's-venture,–Cliff-stile,—Cracking-whole,—Hay-cliff (or Highcliff),—Ladywash,—Little-brookhead, — Little-Pasture, — Merlin's—Shaw-engine,–Tidslow-Rake,—and Twelve Meers.
Flagg, near Mony-ash	Chapel-dale.
Foolow, in Eyam .	Croslow-Rake,—Deep Rake,—Edge-side, — Middle - field, — Moseley-Groove,—and Water-Groove.

Great

Places' Names.	Names of Mines.
Great Hucklow, in Hope	Eyam-edge, — Have-at-all, — Hills-Rake, — Hucklow-edge, — May-Sough, — Mill-dam, — Tidslow-Rake, — and White-Rake.
Great Longsdon, near Bakewell ..	Cackle-Mackle, — Cross-o'-th'-dale-head, — Deep-Rake, — Longstone-edge-venture (and Blakelow-engine) — Robin-wash, — Sallet-hole, — and Silver-hillock.
Griffe, near Brassington	Golconda.
Grindlow, in Hope .	Silence, — Speed, — and Tidslow-Rake.
Grindon, near Buxton, in Hartington	
Hartington	Botany-Bay, — Clay-pit-dale, — Corder-Low, — Long-dale-head, — New-haven-mine, — and Red Mineral.
Hassop, near Bakewell	Deep-Rake, — and Water-hole.
Hopton, near Carsington	Nursery, — and Yoke-Cliff.
Little-Hucklow, in Hope	Maiden-Rake.
Little-Longsdon, near Bakewell ..	Nay-green.
Litton, in Tideswell	
Matlock	Coal-hole-Rake, — Crichman-pipe, — Cross-Rake, — Dimple, — Gentle-woman's Pipe, — Granby, — High-tor-Rake, — Knowle's, — Lady-gate, — Mullet-hill, — Nether-hay, — Nester's Rake, — Old Nester's Pipe, — Seven-Rakes, — and Side-Rake.
Matlock-Bath, in Matlock	Cornel-Rake, — and Cumberland (or Rutland).
Meadow-Place, in Yolgrave	Robinstye.
Middleton, by Wirksworth	Bondog-hole, — Burrows, — Earl of Mar, — Hill-top, — Jackson's, — Middle-ditch, — Noger-hole, — Samuel, — Slack, — Solms, — Spar-Rake, — and Yeild.
Middleton, by Yolgrave	Cross-flat, — and Long-Rake.
Monyash	Highlow-pipe, — Hubberdale-pipe, — and Turnip-close.

Places' Names.	Names of Mines.
Newton-Grange, near Tissington .	
Over-Haddon, near Bakewell	Cow-Close,—Dale,—Mandale,—Robinstye,—Stone-pit,—and Wheel's-Rake.
Overton, in Ashover	Black-stone,—Brimstone-Dyke,—Gregory,—Overton,—and Townsend.
Parwich	
Peak-Forest, near Tideswell	Boston,—Jowl-groove,—Oxlow,—and Portaway.
Rowland, near Hassop	Bright-side.
Sheldon, near Bakewell	Field Rake,—Hard-Rake,—Hubberdale-pipe,—Magpye,—and Wam.
Snitterton, near Matlock	Lea-wood,—and Ox-close.
Stanton-Harold, Leicestershire ..	Stanton-Park.
Stanton in the Peak, near Yolgrave ..	Amos-cross,—Blythe,—Dunshole,—Stoney-Lee,—Wells-Rake,—and Wheels-Rake.
Stanton, near Wooton, Staffords...	Thawswood-Dale.
Stoney-Middleton, near Eyam	High-Field,—Seedlow,—and South-side.
Taddington, near Chelmerton	Horse-steads,—Lees,—and Maury.
Thorpe	
Tideswell	Black-Hillock,—Bull-Rake,—Calvestone,—Chap-maiden,—Clear-the-way,—Field-side,—Hedge-Rake,—High-Rake,—Shuttle,—Thornhill-slack,—Tidslow-Rake,—White-Rake.
Upper-Elkstone, E. of Leek, Staff...	Hill-house,—Mixon,—and Riletch.
Wardlow, near Tideswell	Cowslop,—and Seedlow.
Warslow, in Staff.	Cow-close,—Dale,—Ecton,—and Hay-brook-gate.
Waterfell, in Staff.	
Wensley, near Darley	Barley-close,—Mill-close,—Slack,—Smithfield,—and Windmill.
Wetton, in Stafford.	Bincliff.

Winster,

Places' Names.	Names of Mines.
Winster	Calow,– Coast-Rake, – Lime-kilns (& Drake),–Mossey-Meer,– Orchard-pipe, — Placket-pipe. — Portaway-pipe,—Trafalgar (or Willow),—and Yate-stoop.
Wirksworth	Alport (near Ashley-hay), — Bage,— Barlow-flat, — Biads-Barn, — Blobber,— Boggard, — Bond's-vein, — Dale-top,— Fox-hole,—Goodluck (Burdet's),—Grey-mare, —Holley-hole,—Leas Vein,—Meerbrook, — North-Cliff,—Oakcliff,— Orchard, —Pens-Rake, — Prince-Charles,— Ranter (or Raven-tor),–Ratchwood, —Sand-hole-pipe,—Solms, —Stafford's-dream (or Dream), —Thistley,—Wall-close,–and Yoke-cliff.
Yolgrave	Bacon-close, –– Black-shale-pits, –– Crash-purse, — Hagues (in Callenge-Low),—Long-Rake,––Nick-sough,– and Side-way (in Callenge-Low).

XX. *Rejoinder to the Rev.* C. J. Smyth's *Reply, on Modes of Tuning Keyed Instruments;* (see vol. xxxvi. p. 435.) By M.

To Mr. Tilloch.

Sir, Mr. Smyth, in the Reply with which he has honoured my Remarks, after admitting that he had in one instance reasoned inconclusively, and after explaining that by *wolves* he means *any* chords that are greatly tempered, expresses a wish that I would inform him what the system is according to which I tuned a harp, "so dexterously," by the melody alone. This I will endeavour to do, if you favour me with an opportunity. In the first place, allow me to observe that my sole motive for mentioning the possibility of tuning in that manner, was, to show the power of the practised ear to judge of intervals, and thereby to strengthen my assertion that many, who are mere tuners, are guided, in the operation of tuning, by no theory, by no calculation. And, since I sent you my Remarks, I have seen a very ill-written little book on the subject, by A. Coblenz, a professed tuner and teacher of the art, wherein the author says that to tune "is

"is nothing more than to know how to play a tune right, which, when once learnt by heart right, will be the repetition over again," &c. p. 3. 1797. I have besides met with some tuners who were ignorant even of the possible number of common chords. And as this is the case pretty extensively, I suspect; and as they are seldom musicians, and capable of judging of any other effects of a chord than its smoothness or roughness; surely their opinions and practice are of little weight, when poised against those of such experienced musicians as Rameau, Marpurg, Kirnberger, Kollmann, &c.; not to mention philosophers, again. Mr. S. has declared to us that he is no philosopher, and yet, immediately after, he says it is possible to raise *philosophical* doubts whether a real equal temperament has ever been heard. It is not easy to answer his question respecting that temperament on the organ: I can only say, that I once heard one tuned by a monochord to that system; and I remember that I thought the harmony not very pleasing. If I find leisure at some future period, I design to try that system on my organ, which is well contrived for experiments of this nature. It was built by my father about fourteen years ago; and every metal pipe, except the very small ones, has a short tin slider, in the upper extremity, for the purpose of adjusting it to the pitch required. He made this addition to the pipes, in consequence of an ignorant tuner's having cut them too short for the common pitch. Mr. Smyth has said that, if I had stated my favourite system of temperament, it should have been submitted to examination. That, perhaps, I should have done, could I have perceived what are his principles for judging of the relative value of different systems. Are they the following?

"1. The greater the number is of true fifths the worse is the temperament; for then the small number of fifths, between which the Pythagorean comma is divided, become less supportable.

"2. The case is the same, if the Pythagorean comma is more unequally divided.

"3. The worst temperaments are those wherein some fifths are tempered sharp, because then some other fifths will support, besides the Pythagorean comma, the excess of the sharpened fifths*." Chladni, § 28.

The same author observes, that "As in every thing there is one sole truth and an infinity of errors, so there is

* I quote this from a translation, in manuscript, of Dr. Chladni's last Treatise on Acoustics, 8vo.

only

only one equal temperament, but there are as many unequal temperaments as we please."

The temperament according to which I tuned a harp was nearly the equal. Since reading Mr. Smyth's Reply, I have endeavoured to tune that system on a piano-forte, by the melody alone, striking the finger-keys singly, and in a gradual succession only. I have tried the experiment but once. Twenty-four sounds, near the middle of the general scale of the instrument, were so tuned within fifteen minutes, in the presence of two experienced tuners, who afterwards compared the sounds in one octave with the sounds of a monochord having a scale divided decimally, and found the corresponding lengths of the wire to be as follow:

C 1000, C*939, D 886, D*836, E 793, F 749, F*704, G 664, G*628, A 596, A*559, and B 530; while those of the equal temperament are nearly, C 1000, C*944, D 891, D*841, E 794, F 750, F*707, G 667, G*630, A 595, A*561, and B 530*.

I have no doubt but that, with practice, it would be possible to tune *any* system in the same manner, were it necessary.

Dr. Bemetzrieder, in his silly and useless directions for tuning, says, that the interval called a "coma (comma $\frac{80}{81}$) is the smallest space perceptible by the ear, any thing less is not in its reach." p. 3. I believe, however, that the utmost degree of accuracy of this organ has never yet been determined.

I feel obliged to Mr. Smyth for having, in compliance with my request, given a Table of Beats of the Mean Tone Temperament. Tables of beats furnish the most certain means of tuning any system on the organ, if adapted to the *pitch* of the instrument; but for instruments with wires, they are, perhaps, entirely useless.

The extract from Dr. Burney's History I consider as the least praiseworthy part of Mr. Smyth's Reply. First, it was quite unnecessary, because I did not refer to Eximino's works, but gave his words; merely as an exposition of opinions common with some tuners: Secondly, it was

* D'Alembert, speaking of the equal temperament, preferred by Rameau, says, " Si dans le temperament ordinaire on rencontre des tierces moins alterées que dans celui de M. Rameau, en récompense les quints y sont beaucoup plus fausses, et plusieurs tierces le sont aussi; de manière que sur un clavecin accordé par le temperament ordinaire, il y a cinq ou six modes *insupportables*, et dans lesquels on ne peut rien exécuter." p. 56. *Elemens de Musique*. Lyon, 1779.

not impartial; and it may be fairly supposed that he intended to influence those "who to the fascination of a *name* surrender judgement hood-winked."

I am unacquainted with the Doctor's reasons for giving the term *whimsical* as a translation of *bello*. He goes on to say, "The author (Eximino) has certainly with shrewdness and accuracy started several difficulties, and pointed out imperfections in the theory and practice of music, as well as in the particular theories of Tartini and Rameau." vol. iv. Dr. Matthew Young also, in his Inquiry into the Phænomena of Sounds and Musical Strings, refers to Eximeno, and calls this work of his, *Delle Origine e delle Regole della Musica*, an excellent and admirable treatise. But what has all this to do with the subject of tuning?—I should not have turned from the author's thoughts to search into his general character, if Mr. Smyth had not led the way, and rendered it on my part necessary.

It was my intention to offer a few remarks on Mr. Marsh's Harmonics, (1810,) and on the utility and application of the term *wolf*; but on a second consideration, they do not appear to me of sufficient importance for insertion in the Philosophical Magazine.

A. Merrick.

XXI. *On Oriental Bezoars.* *By M.* Berthollet *.

Bezoars, according to Kæmpfer, were so dear even in Persia, that he did not think a real one ever came to Europe; most of those which are brought are artificial, and some are formed of small fragments of real bezoar joined together by some artful process. I was fortunate enough to examine these concretions under circumstances which admit of no doubt as to their being genuine. Among the presents sent to the emperor Napoleon, by the king of Persia, were three bezoars, which were given to me for the purpose of being analysed. The following was the result of my experiments.

These bezoars were dark-green externally, and brown internally: they were of an oval form, and had a very smooth surface: they were formed of irregular concentric layers: within one of them were found some bits of straw and other vegetable substances, which formed as it were an oval nucleus, a little separated from the external envelope which was nearly two centimetres ($\frac{8}{10}$ inc.) thick: another formed a com-

* *Mem. d'Arcueil*, tome ii. p. 448.

pound

pound mass of layers, among which was a splinter of wood of the size of a common pin: a homogeneous piece was of the specific gravity of 1463, that of water being 1000*.

The bezoar when reduced to very fine powder yields nothing to water in which it has been boiled for a long time: the water, however, was a little greenish, but had no sensible taste. The re-agents did not manifest the presence of any of the substances the existence of which might have been suspected: when evaporated to dryness it yielded scarcely any residue.

Alcohol which was boiled for a long time with bezoar powder also assumed a slight green colour; it was not disturbed by the addition of water: when evaporated to dryness it did not leave any appreciable residue.

Muriatic acid, concentrated in a middling degree, did not act perceptibly upon bezoar; but the concentric nitric acid dissolved it with a brisk effervescence: it assumed an orange-red colour; but we found no oxalic acid in this solution, and we could not extract from it any yellow bitter matter.

Potash easily dissolved bezoar powder: the solution was of a deep brown colour, and the muriatic acid precipitated from it the substance of the bezoar without its undergoing any apparent alteration.

We distilled over a graduated fire twelve grammes of bezoar reduced into fine powder; then passed into the receiver a small quantity of a yellow substance, part of which was sublimed, and the other part was covered with a little liquid, on which some drops of oil floated: the liquid gave indications of acidity, and resembled a weak pyroligneous acid: lime extricated from it ammoniacal vapours, but they were scarcely perceptible.

When we put bezoar on burning coals, it burns, but gives out very little flame: there rises, on the part furthest removed from the coals, a little yellow matter: when we expose bezoar in powder in a small platina spoon to the action of the blow-pipe, it burns briskly, but without flame, and around it this yellow matter is formed, which when afterwards exposed to the flame is charred and burnt. It seems, therefore, that the yellow matter is only a portion of the bezoar not much altered, which is sublimed; and which, when again exposed to the action of the fire, is reduced

* In appearance the above bezoars were very little different from those described by Messrs. Fourcroy and Vauquelin in their paper on Animal Concretions, as *resinous intestinal bezoars*, but a chemical analysis has established that there is a considerable difference between them.

duced into charcoal and consumed like the rest. The twelve grammes of bezoar left in the retort 4·320 grammes of charcoal, which produced upon incineration 0·600 grammes of ashes, which when washed with water left on evaporation a residue formed of crystals, but so confused, and in so small a quantity, that their nature could not be distinguished: they were re-dissolved, and we ascertained by the nitrate of silver, the muriate of barytes, and the solution of platina, that it could be nothing but sulphate of soda with a small proportion of muriate of soda.

The residue from the lixiviation was submitted to the action of the weak muriatic acid: the whole was dissolved with effervescence, excepting 0·086 grammes of silex: the muriatic solution gave by means of ammonia a precipitate, which when collected on a filter and properly dried weighed 0·095 grammes: it appeared to be phosphate of lime; afterwards the carbonate of soda formed a precipitate, which, being dried, weighed 0·151, and which was carbonate of lime: loss 0·098. A second operation gave nearly similar products.

We see by the foregoing analysis that bezoars, such as the above, have no resemblance to other animal concretions, and that they give precisely the products of vegetable substances, and particularly those of wood.

Like wood they yield a great proportion of charcoal, and they present the greatest analogy to it when we submit them to the action of water, alcohol, the acids; but most of all the alkalis. I shall here quote a passage from Thomson's Chemistry, in which he describes the properties of the ligneous substance: "This substance is insoluble in water and alcohol: the fixed alkalis give it with the assistance of heat a deep brown colour: they soften and decompose it: a weak alkaline solution dissolves it without altering its nature, and we may again precipitate it by an acid. This property renders wood susceptible of being easily separated from most other vegetable substances, since there are very few which are soluble in the weak alkaline lixivia."

We recognise, therefore, in the bezoar the ligneous substance with which the animal is fed: this concretion can only be formed in the stomach; for if it were produced in the intestines, we could not find in it pieces of straw in such good preservation: it would have received some alteration in its vegetable nature, and would be impregnated with some animal matter.

We should say that the softened, and as it were dissolved ligneous substance is consolidated again around a body which

which has favoured its separation, and that its molecules have been able to condense and form a closer texture than that of wood, and assume along with the appearance a specific gravity greater than that of stone.

If we consider the nature of the salts which the bezoar has left by incineration, we may conjecture that the shrubs, which were the food of the animal in which it was formed, grew in soils where the base is soda, such as we know to exist throughout all Persia.

XXII. *Reflections on some Mineralogical Systems. By* R. Chenevix, *Esq. F.R.S. and M.R.I.A., &c. Translated entire from the French, with Notes by the Translator.*

[Concluded from p. 51.]

PRACTICAL USE OF THE WERNERIAN METHOD.

If the system and method of external characters had been entitled *The Miner's Guide or Manual*, we should have required from the author only that kind of knowledge which the miner might possess or employ. But how has it been imagined possible to qualify these principles with the name of science, when the auxiliary intelligence of all the collateral sciences has been excluded? In the estimation of specific gravity, in which there are as many degrees as bodies in nature, as we have seen, M. Werner admits of only five general divisions, and takes no notice of the details which constitute individuality. In the measure of angles, where nature has presented so many degrees and combinations drawn from the vast treasure of infinity, Werner despises the instrument which could give us new eyes to estimate them, or supposes its application too difficult for his science. Thus, we are taught what the mathematics have never suspected, that a right angle may be that which is greater than 90° in the ancient division of the circle; and two characters which he himself, and others still more than he, have justly considered as extremely important, are not only reduced to be of no greater value than unctuosity, coldness, &c.; but in studying them, we study errors; we learn to falsify the sciences, whose beauty and accuracy will never be equalled by details capable of filling a whole library on the empirical characters of minerals.

In all that the celebrated author of the system of external characters has done, it seems as if he had had no other ob-

ject than to render his means independent of every thing, even of mineralogy itself, with the certainty of depriving them of all the advantages which an enlightened age was in a condition to furnish him. Most assuredly, had he designed to plunge the subject which he wished to make a science in the most profound barbarism, he could have imagined nothing better than what he has done to succeed.

He whose intellectual faculties give him a right to open to himself a career in the empire of the sciences, is very happy to find it already in a state of advanced culture. He makes use of every thing which has been previously published; and it appears as if all those who preceded him, had only prepared the materials, to which every one hopes to add something, or strives to compose an edifice: while that in the seductive regions of the imagination, we meet only flowers which fade under the hand that first collects them; and the poet of the 18th century may find in the Iliad thoughts which belong to him as well as to the chanter of Achilles, but of which others might question his originality because Homer lived before him.

LITERARY STATE OF THE GERMANS.

Here, were it not conducive to a too long digression, I should make some remarks on the advantages and disadvantages which the Germans have had as well in literature as in science. While England, France, Italy, and Spain had their poets who observed nature, who studied the ancients, and who polished their native tongues, the Germans were not advanced: their true literary epoch took place near the end of the last century; that is to say, two or three hundred years later than that of other nations. They have been the last to reap, and it remains for them only to divide with their predecessors, and to be accused of plagiarism, or content themselves with mediocrity and remaining inferior. The Germans thought to avoid the one and the other rock; others believe that they have struck on both. It is in roving through regions where human thought had never penetrated, in associating ideas which nature had never assimilated, in creating images, in depicting passions which neither the mind nor soul ever knew; it is, in short, by doing what others have neither done nor wished to do, that they believe themselves original;—these are the things in which they make genius consist. Difficulties are never the fashion; nevertheless, genius is the fashion in Germany. The French, it has been said, hunt after wit; they pursue it at least with grace, and often they have not far to run. It is

is more just to say that the Germans hunt after genius; they pursue it for the most part awkwardly, and without success. Dr. Johnson erroneously said of the Scotch, that every one had a mouthful of learning, but that no one had a sufficiency. The Germans have an indigestion of it. These are some of the reasons why their literature is a vagrant, who is passionately fond of surpassing the limits of nature, and their science a pedant, who lives very remote from the urbanity of the present times.

COMPARATIVE RESULTS OF THE SYSTEM OF HAUY AND WERNER.

Let us yet consider these two mineralogical systems under a point of view as to the relation which exists between the magnitude of the means and the results obtained.

Micrology consists in searching the most minute particulars, in order to draw from them even the least consequences. The contrary of micrology is to draw great consequences by means which appear too small to lead to them. We have no word to designate it. It matters not; genius has its own language. I shall give some examples of the one and of the other.

In the primitive rhomboids of *chalasie*, the plain angle of the summit differs 3° 30′ from a right angle: this makes a difference of 7° between the angles formed by any side from one of the rhomboidal bases, with its adjacent sides. This difference prevents the decrements from acting in the same manner on the angles and edges which are not in similar cases; and it augments, if we may so speak, almost infinitely in the secondary forms. Thus, those of chabasie differ entirely between themselves from those of analcime, and from all those produced on a cubic nucleus. This is a great effect produced by a cause which the eyes can scarcely perceive, but which instruments measure.

What a triumph for principles over empiricism, when they banished from the nomenclature the name of cubic calcareous spar falsely given to a variety of carbonated lime! M. Macie proved that this form was not cubic; and the law of decrement on which it depends being determined, M. Haüy demonstrated, by the incommensurable relation which exists between the axis and the perpendicular on the axis in the cube, that this form could not be produced by any regular laws of decrement which act on the primitive rhomboid of carbonated lime. The angles, nevertheless, differ but about 2½° from a right angle: a new example in favour of that quality which is opposed to micrology.

 M. Haüy

M. Haüy having established crystallization as a predominant principle in his system, there was nothing more natural than to found mineralogical varieties on crystallographical varieties. Several persons think it tiresome and too rigorous, that every small new face should make the mineral in which it is found be considered as a new variety of its species. Let us take a familiar example, muriated soda (common salt). See pl. 38 of Haüy's Treatise.—If the angles *A*, *A*, &c. of the primitive cube (fig. 145) are truncated in a manner to produce the faces *O*, *O*, &c. (fig. 146); the two forms in 145 and 146 are two varieties of muriated soda, however small may be *O*, *O*, &c. If *O*, *O*, &c. augment (fig. 146), *P*, *P*, &c. diminish. Let this continue until that *O*, *O*, be as we see p. 9, 147, with the exception of a small portion of *P*, *P*, &c. which remains, and we shall have an octaedron which will appear truncated in the angles as was lately the cube. Finally, let *P*, *P*, &c. totally disappear, and the form will become that of an octaedron. Here are two extreme varieties in muriated soda; the one has six faces, the other eight, and an intermediate variety which has (6+8) 14 faces. In what could micrology have surpassed all this? The faces *O*, *O*, &c. which begin to show themselves on the angles of the cube, are the feeble indexes of a nascent effort which is about to become immense. If, in the first moment, the relation of *O* to *P* is as an infinitely small to a finite quantity, it will soon become that of a finite quantity to an infinitely small. Micrology would stop at intermediate degrees, and seek some relation, perhaps that of equality, perhaps more than one, between *O*, and *P*. Antimicrology would here recognise three grand epochs; that where the effect does not yet exist, that where it does exist, and that where it ceases to exist; the non-entity which precedes existence, the existence which is continued, and the non-entity which succeeds it. It is the weakness of man which makes him perceive in existence a beginning, a middle and an end, which to eternity have no existence.

It has been wished to try the effect of ridicule on a system of mineralogy founded on the integral molecule, on an infinitely small or an imperceptible thing. While that some sorry jesters circulated in Paris little couplets, which were as soon forgotten, on the infinitely small things of the Marquis de l'Hopital, philosophers by their means dispersed over Europe grand results which will never be forgotten.

Micrology would pass an immense time on particulars which are worth nothing; on the colours, for instance, of minerals,

minerals, and on their smallest shades, so that nothing more could be given to considerations of the highest importance, such as the specific gravity, and geometrical forms. Such is the way of the system founded on external characters.

M. Haüy has drawn great and admirable results from one single principle. Werner in employing a multiplicity of precepts has not even told us what is a species. If the system of Haüy may, in some respects, be taxed with micrology, it is with regard to what is the object of the eyes. In that of Werner, micrology is all in the mind.

Let us distinguish generally, between sense and understanding. The growing faces *O O* in the cube of muriated soda may be micrology to the eyes, but they speak forcibly to the mind. He who would dissect a flea would certainly have a micrological labour for the eyes and fingers; but if it had been hence that Hervey discovered the circulation of the blood, who would have accused him of a micrological spirit? Micrology has never been applied to the calculation of infinitesimals; Leibnitz has never been accused of it; and the name of Newton recalls whatever the human species has possessed of true greatness. The eyes are useless in estimating *dx* and *dy*, the mind only can seize them, and he who does it is already imbued with a true sense of their grandeur.

The materiality only of the objects treated by M. Haüy has denied him the vulgar homage which Longinus would have rendered him.

In reducing the two systems which we have just examined to their true object; it appears as if we might say, that the system of external characters by Werner, in the form it is known to us according to the books which have treated of it, is very superior to all which have been presented to the world before it; that it is extremely useful to the miner, that it may satisfy amateurs who limit their wishes to a knowledge how to name stones; that it presents no idea to the judgement, and requires nothing from the understanding; that if it had appeared in an earlier age it would have advanced science, but at a more recent period it can only make it take a retrograde march. It contains some rules, and presents us with a useful code of methodized empiricism.

The system of M. Haüy appears to me to be the science of mineralogy, and all mineralogy; and perhaps we shall one day or other see that it is more than mineralogy.

One of the ideas which has been hitherto entertained in the

the natural sciences is, that unity of composition is accompanied by unity of form. In it we find the harmony of nature, and it strikes the mind as an immutable law.

If I had to advise a young man destined to be a miner, I should say to him, "Go to Freyberg." If an amateur, who exhibited no proof of thought, consulted me, I would say to him, "Go to Freyberg." But to the promising young philosopher, if I wished his philosophy not to be impaired, I would say, "Go *not* to Freyberg."

Let me be allowed to terminate this memoir by some general ideas on the cause of this difference between the two systems, and to inquire why Werner has spoken to artisans rather than M. Haüy who has written for philosophers.

Mechanics in England, chemistry in France, and mineralogy in Germany, are, if we may so speak, the three national sciences. They form a striking contrast with astronomy and optics, which, in consequence of the few occasions which they have of leaving the cabinets of the learned, remain within the pale of philosophy, and are less obscured by errors and vulgar prejudices. The people dispense with a knowledge of the stars; even their movements, their distances, are unknown by them in nations which have extended their observations furthest, and which, celebrated in the records of navigation, require them as guides in the midst of the ocean to pass from one hemisphere to the other. The greater part of old or short-sighted persons, who have recourse to optics to remedy their natural defects of vision, do not reason on the implements which they use. Even the naturalist, quite a philosopher as he may be, who discovers living worlds on the leaf of a plant, is not always an optician; nor is he obliged to be so, the principles of the science remaining between the professor of physics who studies the light, and the astronomer who contemplates the stars.

Let us examine the state of these three branches of knowledge among each of the three nations, and consider them in each under the twofold point of view of art and of science.

In a country where the marvellous progress of enlightened industry has forced them to invent new means to procure new profits, it was necessary to imagine machines which might combine in one point the strength of several arms to œconomize labour and expense, and to place at their disposal almost unlimited resources. Manufactures are the cause of mechanics becoming popular in England, and the fortunate

fortunate rage for machines, so necessary to their flourishing manufactories, is there generally predominant. In all the workshops of this country we see the most ingenious applications of the principles of mechanics, and in no other part do we find so many machines, and so well executed. At different periods esteemed works on machines and mechanics have appeared; but the one in which, for the time it was written, that philosophy which is the appendage of science most prevails; that to which the finest inventions of the human mind, the differential and integral calculus, have contributed, is by Leonard Euler. The analytical mechanics of La Grange, still more profound, and raised to the highest possible degree of generalization, has extended this philosophical spirit; and, while machines are multiplied in England, and contributing to the great object of private interest and national grandeur, a citizen of Basle, and another of Piedmont, without machines or without having under their eye the art of mechanics, but imbued with their philosophy, conceived profound speculations on the science.

The chemical code in its actual form has been established in France near 30 years; and it is among philosophers that it has begun to take root. The observations which founded it were not made by artisans, but by men of learning; and protected against local errors and popular prejudices, it was not obscured in its origin. The work of Lavoisier has no equal in its kind. The chemical statics of Berthollet contain the excellent philosophy of phænomena, without which there is no chemistry. The idea of a *chemical philosophy* is also French, and we owe to that nation the greatest part of what this branch of our knowledge possesses truly philosophical *.

In England, the products of the arts and chemical manufactures, as well as the greatest part of all others, have been carried to a marvellous degree of perfection. In contemplating the operations which daily take place, they are enabled

* It may be necessary to remind the English reader that the above sentences were written in France, where the language of courtesy has long superseded that of simple truth: under such circumstances it would be uncandid to the ingenious and learned author to submit these expressions to his own rigid principles of the logic of science. He has only adopted, as a matter of common civility, the sentiments familiar among a great number of lettered Frenchmen, who are quite as zealous for the glory of their country as for truth. Some of the most distinguished Parisian philosophers, however, are conscious of their obligations to other countries; and one of these observed, with as much truth as politeness, that "*la plus belle découverte de mes compatriotes, c'est d'étudier bien les ouvrages des vrais philosophes Anglois.*"—Trans.

abled to collect a series of phænomena: but the mind more occupied with the interest of the manufacture than that of science, has generally dwelt less on the explanation of facts than on their utility. Some men of learning nevertheless have considered chemistry in a philosophical view, and have copiously distributed over Europe that multitude of materials, accompanied by some theoretical explanations, which the French chemists afterwards collected into the beautiful edifice which so deservedly bears the name of the theory of Lavoisier *.

In Germany there are less chemical manufactures in general than in England; nevertheless, pharmacy, which is preeminently the chemical art, the chemical art properly so called, formerly flourished there to such a degree, that it was proverbially said, that the perfection of therapeutics consisted in an English physician, a French surgeon, and a German apothecary. The analysis of minerals, which is nothing but an art, has been favoured by the same circumstances which disseminated a knowledge of mineralogy in that country. But, in the chemical revolution which has taken place within the last 30 years, the Germans are the only people who have not furnished *one theoretical fact*, (for I do not so designate the discovery of a metal or an earth,) nor an idea to the philosophy of the science. Stahl, Scheele, without mentioning Margraaf and others, merited and acquired a high reputation. But there is nothing to hinder the artist from being sometimes a man of genius, or rather the man of genius is sometimes reduced to be an artist.

Germany includes some of the provinces of Europe the richest in minerals, and we there find the most ancient mines explored. As the government is partly interested in their success, it has believed it equally advantageous to disseminate publicly as much knowledge as possible on this subject, in order that the advantages which should result might be distributed among all persons, and return to itself. England has also very rich provinces; the mineralogical manufactures in this flourishing island have not remained behind others. But individuals being much more interested in their success than the government; each one has sought particular improvements in order to surpass his neighbour, and has taken care to conceal his process. France has somewhat neglected mineralogical and metallurgical works, and cannot be placed in the same rank with respect to the

* The truth conveyed in this sentence fully compensates for the insignificant general compliments previously bestowed.—Trans.

arts

arts which depend on them. Germany possesses numbers of books on the mineralogical art, in which we seek in vain for an idea of philosophy. The English also have their detached and technical works. The French have had a Dolomieu and a Haüy.

I have cited these examples to prove that the philosophy of a science does not always accompany the arts which depend on it. Perhaps we might even say, notwithstanding their apparent relation, that they are often in an inverse proportion to one another, as well among individuals as nations. We might even extend this observation further, and follow it in the moral sciences, and even demonstrate generally and *à priori* that it is an inevitable consequence of the nature of things.

LOCAL ADVANTAGES TO SCIENCE.

If in what I have just observed on these three sciences, we remark any disproportion between the causes and effects, it is because the latter undergo modifications by the organization of society, the division of riches, the relations between necessitous trade and enlightened industry: in short, by all the circumstances which influence the manners and characters of people. In England, for instance, affluence, more general than elsewhere, enables a greater number of persons to live independent and pursue any object at their pleasure. The habit of reflecting, and the respect which the higher branches of knowledge insures, favour philosophical speculations, however manufactures may be of a more direct necessity to the state and more lucrative to the individual. A necessary consequence of this state of things is, that with a much greater number of artisans, we should not find in England fewer philosophers than elsewhere, but that we shall often meet persons who are eminently distinguished in both respects. In France there is less necessity for manufactures and commerce; but society is more distinguished for men of learning, and the government gives them salaries*; there are fewer artisans but not fewer philosophers.

* Were it not foreign to the subject, it would be easy to prove, that the mercenary spirit of pensioners or hirelings is incompatible with the spirit of true philosophy, and that genius is always most advantageously exhibited when creating its own resources. The anniversary addresses of the present enlightened President of the Royal Society have occasionally pourtrayed with the hand of a master, the great superiority of a society supported by the voluntary contributions of its members, over that of a body supported by the charity of the state, and acting in servile obedience to the powers that be. Fortunately, we have no *pensioned* societies; and in our National Museum there is more of Wernerian system than the science of mineralogy. Trans.

sophers. In Germany the sciences are exiled to the universities, and the most flourishing manufacture is that of books: they have many learned, many professors, many pedants, and a multitude of book-makers

The state of mineralogy may now be divined according to the circumstances which have prevailed at its establishment. In those parts where the abundance of minerals and the necessity of art have given it birth, we must expect to see miners more expert than elsewhere. They will have a better *tact* at distinguishing minerals which they detach at every blow of the hammer, and even others which they do not meet with every day. They will soon begin to amass specimens, will form collections, and organize the confusion under the name of science If at length there should arise a man gifted with penetration and sagacity, and who felt the disadvantages of this disorder, he could do much to facilitate the study and the knowledge of minerals; but having in view the interest of miners, his system would savour of it, and leave much after him to be done for philosophy. If he adopted easy means, which neither required great profundity of thought nor a chain of reasoning to be comprehended, we should see multitudes of amateurs disperse themselves over the mountains or plunge into cavities searching for minerals. But if the trade of writer is a resource, if the state of author is a title in society, what part of our knowledge offers so many facilities as mineralogy? Learned men will spring up from its touch like men from the hand of Deucalion; and stones will be thrown in handfuls before, behind, to the right and to the left: they will transpose, arrange, derange, reason or not reason, write and speak without ceasing. Among the amateurs of every kind that Heaven sends, may it above all preserve to us the amateurs of pebbles!

If it be not the necessity of an art which directs the attention to mineralogy, it will assume another form. It will admit principles with which the miner may dispense, and will take care not to become popular. I have heard a person very well versed in the diagnosis of minerals, and who passes for being very able at distinguishing the colour, brilliancy, cold and heat of specimens, say, that without being a Newton one could not comprehend the work of M. Haüy. A very well informed man, the possessor of one of the finest cabinets in Germany, author of a work printed on vellum paper and bound in morocco, maintained to me that the system of Haüy was worth nothing, because his cook had found sugar crystallized in cubes and sugar crystallized

stallized in rhomboids, while that sugar is always sugar. The chief director of the cabinet of a sovereign also did not like it, because Haüy pretended to prove all with a piece the size of a filbert. A director in chief of mines, a supreme and secret counsellor, tired of the ordinary means of carrying off the water which inundated the bottom of a mine, travelled eight posts of Saxony in two days to consult me on the means of decomposing this liquid by sulphuric acid or by fire; and a celebrated professor considered his idea poetical. It is thus that every extraordinary idea, however absurd or ridiculous it may be, is qualified in Germany.

A particular cause has still contributed to the confusion of mineralogy. The animal kingdom is that of the three kingdoms of nature, which, in proportion to its richness, furnishes the smallest number of species useful for the necessities of life, and the characters which distinguish them are marked with precision. It is hence that the knowledge of individuals should precede a very little the philosophy of the science, that is to say, the knowledge of the species. The vegetable kingdom has not been exempt from its portion of enigmas, to which ignorance and charlatanism gave birth. More researches were necessary to find the true specific characters than in zoölogy, and the species furnish more materials for the arts. The labyrinth of mineralogy has been without limits. From him that digged in the earth without ever seeing day, to him that had made a study of this part of nature, each had his system and created names. The resistance which some minerals gave to the instruments employed to raise them determined the species to one, the other knew them only by the value of their contents. Utility gave a distinguished place to such substances; the want of knowledge to offer means made such others be rejected. The habit of seeing minerals gives a certain facility in knowing them; the little precision of their characters gave birth to opportunities for confounding them:—such were the resources in which consisted the philosophy of mineralogy.

In the contracted views of those who exclude from nature every thing of which they do not directly feel the necessity, and in the confusion which results from the varied use of the objects that philosophy should consider, what are the resources which it can derive from the arts? In the shop of the joiner philosophy will see but trees, and those deprived of life. Shall it consult the sculptor on the products of the mineral kingdom? from him it can learn to know but a very small part of the objects to which the science extends. Shall it

it join the knowledge of the architect with that of the joiner and the apothecary? it may surcharge its list of repetitions, and will have seen but a part of what it searches; for the arts are far from embracing all the objects of nature. It will overstep its measure in certain cases, while that in others it will not have done enough to attain it; that is to say, it will totally fail of its end.

But what shall we say is the utility of philosophy, if the arts can exist without it? What are its functions? and why this scaffolding of principles when empiricism alone can suffice our wants?

It is in stripping truth of the deceptive cover under which ignorance has disguised it, that philosophy renders it recognisable. If it goes to consult in their workshops those who have lived only with the immediate objects of their labour; if it questions them on all the advantages which may be derived, and on the process which they employ to incline them to the good of society; if it passes to those who conceive that the universe finishes where their art finds no more resources, or with those who seek the materials which they require in a more extended world, it is in order to collect all that is scatttered among one and another; it is to prune what is superfluous, and to determine what is worthy to be preserved. It is philosophy which illumines the mind, and gives brilliancy to the true light which should guide its progress. It marks to the imagination the true point where the judgement stops, and beyond which illusions mislead it; and if among the phantoms of an exalted mind it perceives some real beings, it assigns to the latter the place which is destined for them in the catalogue of human knowledge.

It is thus that philosophy has elevated some arts to the rank of sciences, that it has perfected some others, and that it protects all against the oversights of ignorance, the attacks of barbarism, and the lapse of ages. The art which has deposited its principles in the archives of philosophy will not perish but with it.

XXIII. *On the Beats of Mr.* Hawke's *Douzeave, or common System of Twelve musical Notes. By a* Correspondent.

To Mr. Tilloch.

Sir, Since I find that the Rev. C. J. Smyth is engaged in calculating and sending you the beats of different Systems of Tuning, I am desirous of contributing my mite in sending

ing you the lengths of strings, vibrations and beats, of the irregular douzeave system of the Rev. William Hawke, which has been treated of, or referred to, in four places in your Magazine (viz. vol. xxvi. p. 171, vol. xxviii. p. 304, vol. xxx. p. 5, and vol. xxxvi, p. 47), without these important particulars having been published; the last of which, the beats, are particularly useful in showing the proportionate effect or annoyance which the ear will receive from every false concord in the scale; at the same time that they furnish an almost infinite number of ways of tuning this system, or of checking the tuning as it proceeds.

I am, sir,

Your obedient servant,

London, Feb. 9, 1811. J. B.

TABLE OF BEATS.

1.	2.	3.	4.	5.	6.	7.	8.	9.
C	·5000·000	480.						
B	·5333·333	450.	13·377	36·891	4·478	3·350	8·933	16·833
♭B	·5597·118	428·7922	54·049	5·333	——	3·192	63·712	10·680
A	·5970·259	400·9925	11·956	5·000	4·000	2·993	7·981	10·012
✻G	·6352·475	377·3055	16·833	30·972	——	✻10·029	14 981	35·748
G	·6683·250	359·1066	10·680	4·466	3·573	2·674	14·240	8·944
✻F	·7128·801	336.6625	10·012	31.856	3·350	2·506	6·683	31·856
F	·7481·399	320·7960	35·748	3·990	3·192	2.388	47·664	7·990
E	·7980·149	300·7463	8·944	7·491	2·993	2·239	5·970	7·491
♭E	·8395·679	285·8614	31.856	7·120	♭10·029	——	36·891	10·693
D	·8932·168	268·6623	7·990	3·341	2·674	2·000	5·333	6·689
✻C	·9528·710	251·8704	7·491	23·832	2·506	——	5·000	27·024
C	1·0000·000	240·	10·693	2·985	2·388	1·787	30·972	5·978
Notes.	Lengths of Strings.	Vibration in 1s	♭3d.	✻IIId.	✻4th.	♭Vth.	♭6th.	✻VIth.
			Beats made in one Second.					

The beatings are all *flat* or *sharp*, as expressed in the titles at the bottom of each column, except the 4th on ♭E, and the Vth on ✻G, which are otherwise expressed.

XXIV. *Communication on Water-Pressure Engines. By Mr.* JOHN TAYLOR.

To Mr. Tilloch.

SIR, IN your Magazine for January, I have just seen the remark of Mr. John Farey on a passage in a former Number relative to an improvement of mine in Water-pressure Engines. He conceives that he has discovered a mistake in

 the

the statement, and quotes an example to prove it. It appears to me that the only mistake in the paragraph alluded to is this: that a relative term is made use of instead of a positive one, and that therefore it may be variously applied by different persons, according to the standards by which they form their comparison of size. Mr. Farey may have been in the habit of viewing small engines; while, in Cornwall and this part of Devon, we are surrounded by large ones.

He says that the author of that paragraph has made a mistake, when, speaking of Water-pressure Engines, he states that "none have yet been successfully made upon a large scale," because, as he says, Mr. Trevithick erected one in Derbyshire, which, by a fall of 144 feet, pumped the water from a mine 48 feet below the sough.

Now, if Mr. Farey and myself were severally to describe this engine as to its magnitude, judging by the effect as he relates it, I apprehend that each of us would appear to another person to make mistakes; for I conceive that he would speak of it as being on "a large scale," and I should certainly call it one on a small scale.

As the quantity of water raised by the engine is not stated, it is possible that it may be somewhat larger than I conceive it to be by the only means he has afforded me of forming any kind of judgement; namely, the depth of 48 feet. But unless the volume discharged by the pumps is indeed much greater than generally occurs in mines at this depth, I should not call an engine which pumps water 48 feet, or indeed almost as many fathoms, one on a large scale.

There are pressure engines in Cornwall of a larger size probably than the one Mr. Farey mentions, on Mr. Trevithick's plan, I believe, as well as on other constructions, and with cylinders of a certain diameter they answer extremely well on the whole. But pressure engines have been generally objected to, by those who have seen most of them, when an attempt has been made to adapt them for copious streams, on account of the difficulties in the action of the pistons, which serve for valves, where a large aperture for waterway is required.

Having purchased one some time since, for a mine under my management, of a size which I conceive must be larger than the one in Derbyshire, and than any other I ever heard of, I found the inconveniences in its action which were apprehended, and it was to remedy these inconveniences that I endeavoured to contrive a new construction for

for the valves. I believe that I have succeeded in doing what I wished, and that the valve which I have invented for the purpose will render it possible to erect engines of this sort for large streams of water, and consequently with cylinders of large diameters, which may act as well as those formerly erected have done with small streams applied with considerable falls.

I have as high a respect for the talents of Mr. Trevithick as Mr. Farey can have, and admire the ingenuity displayed by him in the construction of his pressure engines, as well as in other instances. I believe that the merit of bringing this kind of engine to a considerable degree of perfection may be ascribed to him: but I apprehend he only recommended them in cases where a small stream might be applied with a considerable fall.

It may, however, often occur in certain situations, that it should be desirable to apply a copious stream where the fall may be greater than any overshot wheel can well be built for, or where wheels cannot conveniently be placed: in such circumstances I think my engine would be a useful one.

I shall, however, speak with more confidence on the subject when its merits shall have been more fully tried. Perhaps I may at a future time trouble you with some remarks on the action of this engine, and then Mr. Farey will have an opportunity of comparing it with those which he has seen.

I am, sir,

Your most obedient servant,

J. Taylor.

Holwell, near Tavistock,
Feb. 5, 1811.

XXV. *On the Orbits of the newly discovered Planets. By Mr.* Firminger, *late Assistant at the Royal Observatory, Greenwich.*

Since the discovery of the four small planets Ceres, Pallas, Vesta, and Juno, little seems to have been known in this country respecting their situations. The astronomers on the continent, however, have been more successful, and have not only kept up a constant series of observations on these small bodies, but have, with that indefatigable labour and address in the application of mathematics to every department of science, for which they are so peculiarly eminent, availed themselves of every opportunity to improve the

the elements of their orbits, and have given from time to time more correct ephemeres of their geocentric places. Unfortunately, the political affairs pending between this country and France have precluded us the advantage of their investigations. It is therefore much to be regretted, that none of our own mathematicians and astronomers have imposed upon themselves the laudable and interesting task of furnishing the practical astronomer with such helps as might enable him to observe these planets at least in those positions of their orbits the most necessary to give the best determinations of their respective elements. Plate III, first given by Mr. Bode, but considerably enlarged, represents the relative situations of the orbits of these four planets, with respect to themselves, to Mars, and to the Earth. The dotted parts of the circles represent the southern half of their orbits; and the undotted parts the northern half. The positions of the planets on their respective orbits will be seen from the following tables of reference.

POSITIONS OF CERES.	POSITIONS OF PALLAS.
a January 1, 1808.	1 January 1, 1808.
b July 1, 1808.	2 July 1, 1808.
c January 1, 1809.	3 January 1, 1809.
d July 1, 1809.	4 July 1, 1809.
e January 1, 1810.	5 January 1, 1810.
f July 1, 1810.	6 July 1, 1810.
g July 1, 1811.	7 January 1, 1811.
h January 1, 1812.	8 July 1, 1811.
i July 1, 1812.	9 January 1, 1812.
	10 July 1, 1812.

POSITIONS OF VESTA.	POSITIONS OF JUNO.
k May 2, 1808.	$\varkappa$ January 1, 1808.
l November 2, 1808.	ξ July 1, 1808.
m May 18, 1809.	π January 1, 1809.
n May 29, 1807.	ω July 1, 1809.
o June 19, 1807.	ζ January 1, 1810.
	δ July 1, 1810.
	χ January 1, 1811.
	γ July 1, 1811.
	β January 1, 1812.

N. B.—Mr. Groombridge, whose indefatigable labours have been already so productive of improvements in practical astronomy, has observed Ceres at the last opposition, which

which happened on February 17th. The observed place, which he has done me the honour to communicate, agrees with surprising exactness to the computed one deduced from the last and most improved elements. Mr. Groombridge, I believe, is the only astronomer in this country who has observed this opposition.

XXVI. *Description of a Burning Mirror, by means of which we may reflect and fix on any Object, whether at Rest or in Motion, the solar Rays in as great a Quantity as we please. By* F. PEYRARD, *Professor of Mathematics in the Bonaparte Lyceum. Translated from the French.*

Report made to the Class of Physical and Mathematical Sciences of the French Institute on the above Subject.

M. PEYRARD, who has recently published an elegant translation of the works of Archimedes, naturally turned his attention to the way in which that great geometrician is said to have set fire to the fleet of Marcellus before Syracuse. The ancients and the authors of the middle age assert that he employed a burning mirror; but none of them enter into details ample enough to give us an exact knowledge of his process. Anthemius, who in the sixth century built the church of St. Sophia at Constantinople, and who seems to have been an excellent architect, suggested an assemblage of plain mirrors which might produce the same effect with the mirror of Archimedes. Since that period, Kircher, who perhaps was not acquainted with the works of Anthemius, proposed something similar. Finally, within these few years, M. de Buffon contrived a burning mirror composed of 168 plain glasses, and every person is acquainted with his experiments on the subject. The above three processes, which are all closely alike, are attended with serious inconveniences.

In order that a mirror may reflect upon one point the rays of the sun, regarded as parallel to each other, we know that its reflecting surface ought to form part of that of a paraboloid of revolution, the axis of which is parallel to the rays of light, and the focus of which is a point of union. If the mirror should be composed of a great number of plain mirrors of a middling size, the planes of these last ought to be parallel, each to the tangent plane at the surface of the paraboloid, at the point where it is cut by the corresponding vector ray. Now, in virtue of the sun's motion, the position of the axis of the paraboloid changes in a very rapid

manner. It is necessary, therefore, if the form of the mirror be invariable, that this mirror should turn entirely with the sun around the focus; which appears impracticable: and if the elements of which it is composed are moveable, independently of each other, each of these plain elements must turn in such a way as to be constantly perpendicular to the straight line, which divides into two equal parts the angle formed by the ray of the sun and the corresponding vector ray.

It seems difficult to give to the elementary mirrors the motion in question, by means of a machine, less perhaps on account of the changes of declination of the sun rendering the machine complicated, than because the dilatation of the metallic rods which would transmit the motion, might change in a remarkable and unforeseen manner the directions of the elementary mirrors, and because the inevitable shakings would give to these mirrors a vibratory motion, which would keep the individual images in a perpetual agitation.

There remains therefore no other reasonable method of composing a burning lens of several plain mirrors, than to confide each of them to a person individually instructed how to keep it in the position in which it ought to be in order to reflect the image of the sun on any determinate point, and to vary this position conformably to the motion of the sun. But M. Peyrard truly observes, that this method is attended with an inconvenience which completely opposes its success. It is very easy, in truth, for a single attentive person, conveniently placed, to direct on a given point the image of the sun reflected by a mirror of middling size, and to keep it there in spite of the motion of the sun: nay, the difficulty would not be great for three or four persons to do the same thing at once. But if 50, 100, or 200 persons were to form in this manner a burning focus, as none of them could distinguish the image which he sends from that which is sent by the others,—if a single one among those images was removed from the focus, each of the performers would wish to ascertain if it was his: an agitation and confusion would hence ensue, which would hinder the focus from being formed. It is this inconvenience which M. Peyrard has set about remedying, and he obviates it in a very ingenious manner. With this view he furnishes each of his mirrors with a simple apparatus which we shall now describe.

A small glass supported on a trivet, and furnished with two wires which cross each other at the focus of the glass, may

may be easily directed to any given point. It is kept in this position by two screws. The glass, without changing its direction, is moveable on its axis between two collars, and may be maintained in all its positions around this axis by another screw: it projects outwards from the mirror, which it carries round along with it on its axis, and which, independently of this motion, may turn around a particular axis perpendiular to that of the object glass. We turn the glass on its axis until the particular axis of the mirror is perpendicular to the plane formed by the incident and reflected rays, and we keep it in this position by the screw. Finally, we turn the mirror on its particular axis until the rays reflected are parallel to the axis of the glass, and we are then certain that the image of the sun is upon the object towards which the glass is directed.

The two motions just mentioned are executed one after another, and are susceptible of very great precision. With respect to the first, when the particular axis of the mirror is perpendicular to the plane of the incident and reflected rays, the edge of the frame which is perpendicular to the particular axis of the mirror has a shade which is in a plane parallel to that of the incident and reflected rays, and consequently parallel to the axis of the glass. Thus this shade ought to cut the face of a salient index outside of the glass, in a straight line which is at the same distance from the axis of the glass with the edge of the frame. This straight line being therefore traced on the face of the index, in order to execute the first motion, it is sufficient to turn the glass on its axis until the shadow of the frame of the mirror coincides with the straight line traced on the index; which is of sufficient precision.

With respect to the second motion, it is clear that when the mirror is placed in such a way that the reflected rays are parallel to the axis of the glass, if upon the particular axis of the mirror, and quite close to the edges of the frame, we have rubbed off the plating of the glass on a small piece, this defect in the plating will produce a shadow which will fall on the middle of the straight line of the index. Therefore this middle point being marked before hand on the index, in order to execute the second motion, it is sufficient to turn the mirror on its particular axis until the shadow of the scratched part falls on this point; which is equally precise with the first motion.

We see, therefore, that assistants in any number may each direct the image they severally produce on the point indicated for the focus, without being obliged to observe his neighbours. It may be noticed, also, that the motion of the

sun in its diurnal arc is not so rapid, but that one person may direct ten mirrors close to each other; a circumstance which considerably diminishes the expense and labour attending the operation.

We conclude that M. Peyrard has brought the construction of burning glasses composed of several plain mirrors to a perfection which these machines had not hitherto acquired, and which seems to us to be worthy of the attention of the Class.

Signed Charles, Rochon, Monge,
Paris, 3d August, 1807. Reporter.

The Class approves of the above report, and adopts its conclusions.

4th August, 1807. Signed Delambre,
Perpetual Secretary.

M. Peyrard's Description of his Machine.

This burning mirror consists of an assemblage of plain silvered looking-glasses. Every glass is arranged in the following manner:

An object glass AB (fig. 1, Plate IV.) is moveable on its axis between two collars CC, C′C′, which are fixed with a piece of metal DD.

The small aperture of the glass is at A, and the large one at B: two wires cross each other at right angles at the centre of the large aperture.

A screw E acts on the object glass, and keeps it in the position which we wish to give to it.

The object glass is mounted on a stand like a common object glass, so that we may direct its axis towards a given point: two screws F and G retain it in its position.

The middle of the object glass is surmounted by a cylinder M′ M′, the upper base of which is parallel to the axis of the object glass.

A branch of iron HHH, wrought square, is fixed with the object glass.

A framed looking-glass turns on two pivots MM, OO. The straight line which passes through the centre of the pivots is tangent to the posterior face of the mirror, and perpendicular on the axis of the object glass.

The black trace NN, which is occasioned by the scratch in the plating, is divided into two equal parts by the axis of the mirror.

The large aperture of the object glass is surmounted with a plate of metal which is fixed with it. Before this plate is a square plate ZZ, on which are traced the straight lines XX, YY, which intersect at right angles. The square plate

plate has a stalk which traverses a square hole made in the fixed plate. The square plate may be moved from right to left, and lowered or raised: a screw placed behind the fixed plate keeps the moveable plate in the position which we wish to give it.

The moveable plate ought to be placed in such a way that the straight line XX if prolonged passes by the axis of the object glass, and is parallel to the particular axis of the mirror, and so as that the distance from the straight line YY to the axis of the object glass is equal to the distance from the straight line IK to this same axis. The plate *ZZ* being so placed, it is evident that the straight line YY will be parallel to IK, and that the straight line drawn from the point at which the axis of the mirror cuts IK to the point at which XX cuts YY, will be parallel to the axis of the object glass.

The piece QQ′ is a spring fixed at Q′ with the square. This spring is traversed at Q by the screw RQ. On turning this screw, the extremity of the square presses the pivot OO on the frame of the glass.

The square HHH is surmounted by an assemblage of pieces represented in fig. 2. The piece *a b* and the pivot OO are ranged in an invariable manner. The extremity of the square and the piece VV have a square hole which receives the pivot OO. When we turn the screw T, the piece *a b* may be moved before or behind; and when we turn the screw S, the piece VV may be moved from right to left with the piece *a b*.

In order to give the axis of the mirror a position perpendicular on the axis of the object glass, and to place the moveable plate ZZ (fig. 1.) in such a manner that the straight line drawn from the point at which the axis of the mirror cuts the line IK, to the point at which XX cuts YY, may be parallel to IK, and finally, in order to place the straight line YY parallel to IK, I act in the following manner:

I place the mirror in such a manner that the straight line IK cuts at right angles the axis of the object glass. I turn the screw T, and bring the lower edge of the frame to be tangent to the circular surface M′M′, which is parallel to the axis of the object glass. I afterwards turn the screw T in order to fix the piece *a b* (fig. 2.) in an invariable manner.

I afterwards direct the axis of the mirror on a point of a plain surface placed at a certain distance. This point must be in the vertical plane which passes by the eye of the observer and by the centre of the sun, and this plane must be perpendicular

perpendicular on the plain surface which we have mentioned. By this point, I draw a horizontal straight line, and setting out from this point, I take a second point which is as far from the first as is the centre of the mirror from the axis of the object glass. I unscrew at S. I turn the object glass on its axis; the mirror also on its particular axis, and I advance or retract the piece VV until the centre of the image reflected falls on the second point. I fix the piece VV. I afterwards place the piece ZZ in such a way that the shadow of the straight line IK falls on the straight line YY, and that the shadow of MM may be divided into two equal parts by the straight line XX, and I fix the piece ZZ.

The mirror being thus mounted, it is evident that, whatever be the point on which we may have directed the axis of the object glass, the shadow of NN, and consequently all the rays reflected by the surface of the mirror, will be parallel to the axis of the object glass, provided the shadow of IK falls on YY, and the shadow of NN is divided into two equal parts by the straight line XX.

The mirror being thus arranged, the following is the method of using it:

In order to bring the image of the sun on any given object, we must first direct the axis of the object glass on any given point of the object; 2dly, turn the object glass on its axis until the shadow of the line IK falls on the line YY; 3dly, turn the mirror on its particular axis until the shadow of the band MM is divided into two equal parts by the straight line XX.

These three operations being finished, it is evident that the image of the sun will fall on the given object; or, to speak more correctly, the centre of the image reflected, instead of being on the point of the object upon which we have directed the axis of the object glass, will be at a distance from it equal to that which is between the centre of the mirror and the axis of the object glass.

If in prop rtion as the sun advances we take care to keep the shadow of the straight line IK on the straight line YY, and the shadow of NN on the straight line XX, so that the straight line XX may divide the shadow of NN into two equal parts, it is evident that the image will preserve its first position as long as we please.

Let us now suppose that we have a great number of these mirrors; that they are placed by the side of one another in rows above each other; and let us suppose that each of these mirrors is directed by a single individual. It is evident that the images reflected by the mirrors will be brought

brought upon the same object, and that they will remain fixed upon it as long as we please.

I have said that it will require as many persons as there are mirrors; but it is easy to foresee that a single person might easily direct ten or even twenty mirrors, without having reason to apprehend any displacement of the focus, or the dispersion of the images.

If the object on which we wish to bring the images of the sun was in motion, it would be necessary that each mirror should be directed by two persons: one would be instructed constantly to direct the axis of the object glass on the thing in motion, while the other would be instructed to throw the shadow of the straight line IK on the straight line YY, and the shadow of the pivot NN on the straight line XX, so as that this straight line might divide the shadow of the pivot into two equal parts.

Such is the burning mirror which I have contrived. The construction is very simple; the method of using it is easy; and it is beyond doubt, that by its means we may reflect and fix on an object at rest, or in motion, the solar rays, in as great a quantity as we please.

I shall now show what are the effects which my mirror is capable of producing.

Buffon ascertained by several experiments, that the light of the sun reflected by a looking glass did not lose, at short distances, more than one half by reflection; that it lost, at great distances, scarcely any of its force from the thickness of the air which it had to pass through; and that its force was diminished solely in an inverse ratio to the augmentation of the surfaces which it might occupy upon planes perpendicular on the reflected rays *.

This being granted, let us suppose that the glasses of every mirror are each of them five decimetres high and six broad. I take them to be greater in breadth than height, in order that the images reflected may have their height nearly equal to their breadth; for the rays of the sun being always perpendicular on the axis of every glass, while they are more or less inclined on the line IK, if the height of the glasses was equal to their breadth, when the rays of the sun were not perpendicular on the plane of the glasses, the heights of the images of the sun would be always smaller than their breadths.

In order to calculate with more facility the effects of my mirror, I suppose that the glasses are of a circular form,

* See the Supplement to Buffon's Natural History, 4to edit. Paris 1774, tome i. p. 401 and 405.

having

having a diameter of five decimetres, and that they receive perpendicularly the solar rays. The images reflected by the glasses of my mirror being larger than the images reflected by these circular glasses, it is evident that my results will be something too small.

The apparent diameter of the sun being 32 minutes, it is evident that every point of a glass reflects a luminous cone, the section of which through the axis forms an angle of 32′.

This being granted, let AB, fig. 3, be the diameter of a circular glass, and let this diameter be five decimetres. Let us suppose that the straight line CD, drawn from the centre of the sun on the centre of this glass, is perpendicular on its plane. By the straight line AB and by the straight line CD let us draw a plane, and let the straight lines AE, BF be the intersections of the secting plane and of the surface of the bundle of light reflected by this glass. If the straight lines EA, FB are prolonged, they will meet in a point G, and form an angle of 32 minutes. In fact, the apparent diameter of the sun being 32 minutes, each point of the glass necessarily reflects a luminous cone, the section of which by the axis forms an angle of 32 minutes. Let the straight line HA be the axis of the luminous cone reflected by the point A of the glass, and the right line KB the axis of the luminous cone reflected by the point B. It is evident that the angles EAH, FBK, will be each 16′. But the angles EAH, FBK, are equal to the angles EGC, FGC, since the three straight lines HA, CG, KB, are parallel; therefore the angle EGF is equal to the sum of the angles EAH, FBK, which are 32′. Therefore the angle EGF is 32′.

It now remains to calculate at what distance from the mirror the image reflected will be double, triple, quadruple, &c. to the surface of the reflecting glass. For this purpose I first calculate the distance GD, making this proportion: tang. AGD : R : : AD : GD; or rather, tang. 16′ : R : : 0 $^{\text{metre}}$,25 : GD; and I find that GD is 53$^{\text{m}}$,72.

I afterwards try at what distance from the glass the reflected image is double, triple, quadruple, &c. to the surface of the glass. Let us suppose that it is double in LM, triple in NO, quadruple in EF, &c.

In order to find the distances DP, DQ, DC, &c. I conduct myself as follows:

In order to find DP, I form this proportion:

$\overline{AB}^2 : \overline{LM}^2 : : \overline{GD}^2 : \overline{GP}^2$; or rather $1 : 2 : : (53^{\text{m}},72)^2 : \overline{GP}^2$; on account of $\overline{AD}^2$ being the half of $\overline{LM}^2$, when the surface of the glass is the half of the image reflected.

Knowing

Knowing the value of $\overline{AP}^2$, I take its square root; from this root I cut off GD, *i. e.* $53^m,72$, and I find $22^m,25$. Whence I conclude that the image reflected is double the surface of the glass when it is removed $22^m,25$.

In order to find the distance DQ, we should make this proportion : $1 : 3 :: (53^m,72)^2 : \overline{GQ}^2$. In order to find the other distances, we ought to conduct ourselves in a similar manner.

I have calculated these distances, and I find the following results:

The Image being	The distance is
Double	$22^m,25$
Triple	39 ,33
Quadruple	53 ,72
Quintuple	66 ,41
Sextuple	77 ,86
Septuple	88 ,44
Octuple	98 ,22
Nonuple	107 ,44
Decuple	116 ,16

It is almost unnecessary to say, that these distances would be double, triple, quadruple, &c. if the diameters of my glasses, instead of being five decimetres, were ten, fifteen, twenty, &c. decimetres.

Let us suppose a certain number of my mirrors, and suppose that at a very small distance the images of these mirrors united on the same object are capable of producing a certain degree of heat. It follows, according to the results which I have obtained, that in order to produce the same degree of heat at a distance of $22^m,25$, $39^m,33$, $53^m,72$, &c., we must double, triple, quadruple, &c. the number of mirrors. It also follows, that at one of the distances calculated above, we may produce a heat at least equal to that which would be produced by the heat of the sun, repeated as often as we pleased.

But how many times must we repeat the heat of the sun in order to boil water, set fire to wood, or melt, calcine and evaporate metals, &c.? These questions are not yet resolved. By means of my mirror they might. And in order to gratify in some measure the curiosity of my readers, I shall try to resolve some of these questions, taking as a basis the experiments which Buffon made with his burning mirror.

The glasses of which Buffon's mirror was composed were each six inches high by eight broad. In order to simplify

simplify the calculations, I shall in the first place suppose that, when Buffon made his experiments, each of the glasses of his mirror produced an effect equally great with what would have been done by a circular glass of the same surface, on which the solar rays would fall perpendicularly. I shall afterwards suppose, that all the images reflected by the glasses of his mirror were applied exactly upon each other.

But it is beyond a doubt, that each of the glasses of Buffon's mirror produced an effect smaller than that which would have been produced by a glass on which the solar rays would have fallen perpendicularly; for, the solar rays falling obliquely on the glasses of his mirror, it is evident that the quantity of the rays reflected was smaller than it would have been if the solar rays had fallen perpendicularly on the glasses; and I shall presently show that with Buffon's mirror it is impossible to throw precisely the images of the sun upon each other. It follows therefore, that by taking as a basis the experiments of Buffon, my results will be too great.

On the 23d of March, at mid-day, Buffon at a distance of 66 feet set fire to a plank of tarred beech-wood, with forty glasses, the mirror forming with the sun an angle of nearly 20 degrees of declination, and another of more than 10 degrees of inclination.

On examining the table in a preceding page, we shall find that at this distance the image was quintuple the surface of the mirror. Thus the fifth part of 40 glasses, *i. e.* eight glasses, would have produced the same effect at a very small distance. But at a very small distance the heat of the image reflected is the half of the heat of the sun; therefore, four times the heat of the sun would set fire to a plank of tarred beech-wood. I suppose in this experiment, as well as in those which follow, that the number of glasses only was employed necessary for producing inflammation or fusion.

On the same day, the mirror being placed still more disadvantageously, he set fire to a plank tarred and sulphured 126 feet distant, with 98 glasses.

At this distance, the image reflected was nearly twelve times as large. The heat necessary therefore to set fire to this plank would be the heat of the sun mutiplied by $\frac{98}{2 \times 12}$, *i. e.* the heat necessary for that would be equal to four times and $\frac{1}{12}$ the heat of the sun.

On the 10th of April, in the afternoon, with a clear sun, a tarred plank was set fire to at the distance of 150 feet, and

and with 128 glasses. The inflammation was very sudden, and it took place throughout the whole extent of the focus.

At this distance the image was nearly 15 times as large. The heat necessary, therefore, for setting fire to this plank would be the heat of the sun multiplied by $\frac{128}{2 \times 15}$; *i. e.* the heat necessary would be equal to four times and $\frac{4}{15}$ the heat of the sun.

On the 11th of April, at a distance of 20 feet, and with 21 glasses, a beech plank was set fire to which had been already partly burned.

At this distance the image was nearly double. The heat necessary, therefore, for setting fire to this plank was the heat of the sun multiplied by $\frac{21}{2 \times 2}$, *i. e.* by 5 and $\frac{1}{4}$.

The same day, at the same distance, with twelve glasses, some small combustible substances were set fire to. The heat necessary, therefore, for setting fire to them was the heat of the sun multiplied by 3.

On the same day again, at the same distance, and with 45 glasses, a large pewter flask was melted which weighed about six pounds. The heat necessary, therefore, was the heat of the sun multiplied by $\frac{45}{2 \times 2}$, *i. e.* by 11 and $\frac{1}{4}$.

With 117 glasses, some thin pieces of silver were melted, and a piece of sheet-iron was made red hot. To produce this effect, therefore, there must be a heat equal to that of the sun multiplied by $\frac{117}{2 \times 2}$, *i. e.* by $29\frac{1}{4}$.

"By subsequent experiments," says M. Buffon, "I ascertained that the most advantageous distance to make conveniently with these mirrors experiments on the metals, was 40 or 45 feet. The silver plates which I melted at this distance with 224 glasses were very clean, so that it was impossible to ascribe the very abundant smoke which issued from it, to grease or to other substances which the silver might have imbibed, and as those persons persuaded themselves who were witnesses of the experiment: I repeated it however on plates of silver quite new, and had the same effect. The metal smoked very abundantly, sometimes during eight or ten minutes before being melted. I had intended to collect this smoke by means of a head similar to what is used in distillation, and I always regretted that my other occupations prevented me; for this way of extracting water from the metal is perhaps the only one which

which we could employ: and if it is said that this smoke, which appeared to me to be humid, does not contain water, it would at any rate be useful to know what it is, for it may be merely volatilized metal: besides, I am persuaded that, by making the same experiments on gold, we shall see it smoke like silver, perhaps more, perhaps les."

At the distance of 40 feet the image is triple: the heat necessary, therefore, for producing this effect is equal to that of the sun multiplied by $\frac{224}{2 \times 3}$ *i. e.* by 37 and $\frac{1}{3}$.

Thus, by setting out from the imperfect experiments of Buffon, five times the heat of the sun would be more than sufficient for setting fire to tarred planks. I suppose that eight times this heat is sufficient for setting fire to all kinds of wood, and surely so much heat would not be requisite.

It follows from this supposition:

1st, That at a distance of $22^{m},25$, it would require 16 of my glasses to set fire to wood.
2d, At a distance of $39^{m},33$, it would require 24.
3d, At a distance of $53^{m},72$, it would require 32.
4th, At a distance of $66^{m},41$, it would require 40.
5th, At a distance of $77^{m},86$, it would require 48.
6th, At a distance of $88^{m},41$, it would require 56.
7th, At a distance of $98^{m},22$, it would require 64.
8th, At a distance of $107^{m},44$, it would require 72.
9th, At a distance of $116^{m},16$, it would require 80.
10th, At a distance of 1250 metres, *i. e.* a quarter of a league, it would require 590*.
11th, At half a league, it would require 2262.

If the height and breadth of the glasses became double, triple, quadruple, &c. it is evident that they would inflame at double, triple, quadruple distances. Thus 590 glasses of a metre in height would produce the same effect at half a league, and glasses of two metres in height at one league: but I deceive myself, the effect would be much greater.

If we used glasses of a metre in height, the focus at a distance of a quarter of a league would be 24 metres in height and in breadth. I am of opinion, that with 590 glasses five decimetres high we might reduce to ashes a fleet at the distance of a quarter of a league; at half a league, with 590 glasses of a metre in height; and at a league, with 590 glasses two metres in height.

* In order to calculate how many glasses are requisite at this distance, we form the following proportion:

$$(53^{m},72)^{2} : (53^{m},72+1250^{m})^{2} :: 1 : x^{2}.$$

and we find for the fourth term 590 minus a fraction.

Instead

Instead of employing glasses which should be two metres in height, we might employ four glasses of a metre in height, which we might arrange in the same way, and the effect would be the same.

Before concluding, I shall say something of the burning mirrors which have been contrived to produce effects at great distances. Buffon's mirror was the last I know of. This instrument is composed of 168 plain glasses mounted in iron frames. These glasses, which are six inches high by eight broad, are moveable in every direction.

The above mirror has two prominent defects. It requires about half an hour to adjust it, *i. e.* to bring to the same point the 168 images of the sun reflected by the glasses. But the glasses being adjusted by each other, and the images reflected removing every moment from their first positions, it is evident, that when the operation is concluded the images must necessarily have removed from the focus. Hence it follows, that at every second the focus is displaced or enlarged, and loses its activity.

Let us suppose, for a moment, that, the mirror being adjusted, the images of the sun are exactly applied upon each other: I assert, that in this case M. Buffon's mirror has all the properties, and nothing but the properties, of a parabolic mirror composed of plain glasses.

Let us suppose in fact a certain number of plain glasses BC, DE, &c. (fig. 4,) placed as we please, provided their centres GH, &c. reflect the solar rays IG, KH in a point F. By the point F draw the straight line AL parallel to the solar rays IG, KH; on this parallel take a point A on the prolongation of LF, and describe a parabola MAN, the origin of whose axis is the point A, and the focus is the point F.

If this parabola makes a revolution around its axis, it will describe the surface of a parabolic conoid. Let us now suppose that the glasses BC, DE, &c. approach to or remove from the point F by moving parallel to themselves, following the straight lines GF, HF, until they are tangent to the conoid. It is evident that the points of contact will be the centres of the glasses, and that the centres of these glasses placed at *b c*, *d e* will reflect the solar rays OH, PG, &c. at the point F, in the same manner as they would reflect the solar rays IG, KH, &c. when these glasses were placed at BC, DE, &c. I conclude therefore, that if, Buffon's mirrors being adjusted, the images were exactly applied upon each other, this mirror would have all the properties, and no more than the properties, of a parabolic mir-

ror composed of plain glasses. But a parabolic mirror only reflects the solar images in a single point, when the axis is directed to the centre of the sun: therefore, in order that the images reflected by M. Buffon's mirror may remain exactly applied on each other, it would be be necessary that the axis of the mirror, passing always by the same focus F, should be constantly directed to the centre of the sun. But M. Buffon's mirror remains immoveable during the experiment: thus, in proportion as the sun advances the focus changes place. The mirror in question would therefore have another essential fault, even if the first did not exist.

[To be continued.]

XXVII. *Notices respecting New Books.*

A Treatise on the Venereal Disease, by JOHN HUNTER; *with an Introduction and Commentary, by* JOSEPH ADAMS, *M. D. Author of* "Observations on Morbid Poisons, &c."

MR. HUNTER'S Treatise having already gone through two editions, we should not have thought it necessary to notice a third, were it not for some circumstances which particularly increase our interest in the present performance. The obscurity of that celebrated but almost uneducated author has very much lessened the value of most of his writings. This has rendered an interpreter necessary, an office for which no one could be fitted who was not in frequent habits of conversing with him. Mr. Home may boast advantages of this kind beyond any other person; but probably on account of the multiplicity of his engagements, the public has received little or no information from him which might lead to an elucidation of the most original parts of Mr. Hunter's discoveries. It is probable, indeed, that having received his education entirely from that source, he may not be aware of those points, which to others who have had fewer advantages are particularly obscure. Hitherto we believe Dr. Adams is the only person who has written with the professed object of illustrating Mr. Hunter: and it is but justice to admit, that since the appearance of the first edition of "Morbid Poisons," the opinions of his master have been more generally received, and even his language has become more current.

A new edition of this treatise therefore being called for, we cannot but consider its value much enhanced by the commentaries of such an editor. But the object for which

we

we particularly wish to introduce the present article, is to vindicate the character of the nation from a foul reproach, and the profession of medicine from that uncertainty with which every wag has too often and sometimes too justly charged it.

That the islands of Otaheite first received a deadly poison from the Europeans has been so generally admitted, that the only doubt has hitherto been, which of two rival nations was the author of such a boon: meanwhile, nothing has been so common as to lament the fate of the unhappy islanders, whose very existence seemed in danger by their gradually diminished numbers from this baneful source. How much will our readers be surprised to find that the disease has never existed in those islands! and how much does it redound to the credit of the medical art, that this discovery should be made from imperfect records, which describe the effects of the malady among the islanders without the least doubt or reserve!—For the development of this fact we shall transcribe the passage in the work before us.

"Having thus (says the editor) I hope sufficiently illustrated Mr. Hunter's explanation, how the same matter applied to different surfaces may produce different effects, the reader must indulge me with a few words on the South Sea disease.

"About the year 1800, a lady of fashion, who was recommended to my care in Madeira, brought with her the French account of De la Peyrouse's voyage. Though I had leisure enough to peruse the whole, yet the letters of his surgeon attracted my particular notice. After examining them with the greatest attention, I could not help remarking, that he wrote of *mals veneriens* without the precision of a Hunter. In the end, I was convinced there was reason to doubt whether De la Peyrouse's surgeon had met with the venereal disease in any of the places in which he spoke of it with so much freedom. This induced me to examine the accounts of Captain Cook's voyages, and the result was, a thorough conviction that, if the disease existed at all in the South Sea islands, there was at least no satisfactory proof of it. Under this impression, I wrote to three physicians in London, explaining my doubts, and, perhaps with more quixotism than prudence, was willing, if encouraged, to make a voyage in order to ascertain a point involving not only an important medical question, but in some measure the national reputation.

"Fortunately, this question has been much better decided by one who candidly admits his arrival at those islands with a most perfect conviction that the disease existed there in

all its forms. His inquiry was not, therefore, whether he should find it, but how general, and with what severity, it would appear, and also how he might preserve the health of his crew. From these circumstances, and still more from the character of the gentleman, no doubt can be entertained of the faithfulness of his conclusion, which is, that "*the venereal disease is unknown in Otaheite.*"* At first sight, it may seem strange that this opinion of mine has never been published before the fact was confirmed by Mr. Wilson. To this I can only answer, that to offer an opinion on a subject without the means of ascertaining it, must at least be premature. There are, however, fortunately, witnesses that such was my opinion. Dr. Garthshore is one of the gentlemen to whom I wrote from Madeira on the subject. The late Dr. Pitcairn was another; which is confirmed by his note now in my possession, and also by a communication he made to a most distinguished philosophical character now living.

"But perhaps it may be asked, Admitting the whole as I have stated it, why should the reader be troubled with the account? In order, I answer, that he may learn there are certain characters by which the venereal disease may be distinguished with certainty; that these are so well marked as to be understood by description; and that even the absence of them may be ascertained by those who take the trouble of examining with sufficient diligence. It may then be asked, How could the accurate Hunter have fallen so easily into the belief, that the venereal disease was known in all its forms in the South Sea islands? Mr. Hunter, it may be answered, had not the prolixity of a French surgeon's account to make him doubtful on the subject. When I speak of prolixity in this case, it is not from disrespect. Though De la Peyrouse's surgeon was mistaken, still his descriptions are so minute as to enable the reader to comprehend what symptoms were present. It was from the description, not from the name, of the disease, that I suspected De la Peyrouse's cases were not venereal, and it was natural to transfer this scepticism to the South Sea disease. On examination, it was found that the account, defective as it is, would authorize the same conclusion."

* "From the foregoing statement," says Mr. Wilson, "it may be concluded, without, I hope, presuming too much, that notwithstanding the melancholy accounts we read of the ravages of lues venerea at Otaheite, and even disputations about its first importers, this disease was not introduced there antecedent to the Porpoise's voyages."—See *Edin. Med. Journal*, vol. ii. p. 283.—The Porpoise is His Majesty's ship of which Mr. Wilson was surgeon, and arrived at Port Jackson in June 1801.

We

We shall close this article with a short extract from the preface, explaining the commentator's object in producing this edition.

"It cannot be wondered," says he, "if doctrines entirely new should require a language in many respects new also. What appeared, however, a new language was for the most part only the introduction of precise terms instead of figurative expressions. If it should seem strange that any difficulty should attend describing a plain matter of fact, or in understanding such a description, let us recollect, that in every art or science the great difficulty is to delineate nature, and that few but adepts are alive to the nicer and most accurate parts of such delineations.

"Mr. Hunter found himself so frequently ill understood, that at last he was prevailed on to believe there must be some incapacity about him in the use of common language. That he was totally unacquainted with those ornaments in writing or speaking, which serve to illustrate a subject, or to awaken the attention, cannot be questioned; but his language was always as perspicuous as might be expected from the clearness of his conceptions. This language, however, was not popular; and I believe, if we except his posthumous production, every thing he wrote for the public was revised by his friends.

"The Treatise on the Venereal Disease was the work which he was particularly anxious should come before the world in the most perfect form: 'I am resolved,' said he to his Commentator, 'that it shall not be a mere bookseller's job, every subsequent edition rendering the former useless. The truth of the doctrines I have proved so long as to reduce them to conviction; and, in order to render the language intelligible, I meet a committee of three gentlemen, to whose correction every page is submitted.' As all this was very generally known, never were expectations raised higher of any work, nor in some respects more generally disappointed.

"To compliment Mr. Hunter's coadjutors would be superfluous. Two of them, being authors, have convinced the world of their abilities in producing original compositions. Of the third, it is enough to say he was Dr. David Pitcairn. But these gentlemen, accustomed to the best company, that is, to each other, and to a circle as enlightened as themselves, were not aware of the difficulties that attended their undertaking. To make Mr. Hunter intelligible by the short introduction prefixed to this work, never could have entered the conception of men who were

not previously accustomed to converse with him. It may perhaps be fair to add, that being all of them physicians, they were less acquainted with the erroneous opinions and practices, and even with the technical language, which had prevailed before Mr. Hunter taught.

"Though what has been said may be a sufficient apology for the commentaries offered in this edition, yet it did not seem to authorize any alteration in the text. The value of the work will infinitely more than repay the labour of studying it with all the application it requires. The object of the commentator is only to direct the student, and to relieve him occasionally in his progress. With these views, the following hints are premised."

Some very useful though general hints, on the mode of studying Mr. Hunter's writings, conclude the preface.

To say the work is well executed would be almost superfluous. Our readers must be aware of what they are to expect by Dr. Adams's former writing, and we can promise them they will not be disappointed. Though the book is increased nearly one-fourth, yet the price is diminished a third. We are glad to see this laudable attempt at increasing the circulation of Mr. Hunter's productions, and rendering them more generally intelligible.

An Essay on the Nature of Scrofula, with Evidence of its Origin from the Disorder of the digestive Organs. Illustrated by a Number of Cases successfully treated, and interspersed with Observations on the general Treatment of Children. By RICHARD CARMICHAEL, *Surgeon.* 8vo. Callow, London.

A few months ago we noticed with much satisfaction the valuable remarks of this author on Cancer, and we again with pleasure take the opportunity of reminding the faculty how much the practice in that melancholy disease has been directed by Mr. Carmichael's ingenious suggestions. If we cannot speak in the same terms of the Essay on Scrofula, we must at least admit that the work has considerable merit. We could hardly indeed expect in so short a period, from the same author, two complete treatises on two stubborn diseases, and we almost regret that the attempt has been made. The book, however, is short, and we are ready to hope that it will only prove introductory to something more worthy of the writer.

Having said thus much, it will not be expected that our remarks should be protracted. We shall, indeed, rather offer

offer a few hints for the government of Mr. Carmichael in his future researches, than impose on ourselves the painful task of minutely pointing out the errors or omissions of the present.

First, we wish in this, as in all other books on any branch of philosophy, that the beginning should comprehend a precise definition of the subject, and an explanation of the order in which it is to be treated. What is scrofula? we would ask. Is it any local disease for which we have no other name?—Is it altogether a glandular disease? or, what is it? If it is usually attended with a low state of health, is the disorder of the digestive organs the cause or the effect of the diminished action and strength, which is equally visible in all the other organs? If it always arises from a want of proper diet, proper exercise, or wholesome air, why does it ever attack individuals of those families in which we never can suspect any such causes? or why is it not more uniform in those seminaries, or even in that class of life, where the conditions and habits of so many, if not precisely, are at least nearly similar?

We shall not enter into any discussion on the authorities which our author has collected with equal industry and fidelity; nor shall we dispute the accuracy of his information in the relation he gives of his own observation. Suffice it to say, we have met with many judicious, though not all of them new observations on the subject of nursing; but they only prove, what must be admitted, that the same causes which induce general debility induce indigestion, hardened and suppurating glands intractable in every stage, and only to be healed by improved diet, exercise, and air.

We learn with pleasure, from Dr. Hutton, that the long expected new edition of his Dictionary of Mathematics and Philosophy is ready for the press, and may be expected to be delivered to the public in due time, with all his own improvements, made from the late discoveries in those sciences, for the collecting of which he has assiduously employed himself ever since the time of the first publication of that valuable work.

The first volume of the Transactions of the Geological Society, in 4to. with many plates, is in the press, and will be ready for publication in the month of May next.

XXVIII. *Pro-*

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

Jan. 31. The right Hon. President in the chair. A paper by Mr. Home was read on the non-conducting powers of the thoracic duct and the spleen from the stomach to the bladder. Mr. H. related a number of experiments made with ligatures, or by removing the spleen and keeping a ligature on the thoracic duct, in which state rhubarb was taken into the stomach of the animal, and was detected in the urine; whence he inferred, contrary to some opinions he formerly published, that those two organs are not necessary to the secretion of urine.

Feb. 7. Mr. Smithson's paper on zeolite was read. This ingenious mineralogist having received some specimens of this mineral from M. Haüy himself, and labelled by his own hand, he deemed it a favourable opportunity of ascertaining if there were any chemical difference between the mesotype of the French crystallographer, and zeolite or the natrolith of Klaproth, as he had previously discovered the existence of soda in all the specimens of zeolite which are found in these kingdoms, as well as those in Germany. M. Vauquelin analysed several specimens of zeolite without discovering any traces of soda; but Mr. Smithson discovered alkali even in the mesotype sent him by M. Haüy, and in every other specimen of zeolite in his possession. From this circumstance he is inclined to prefer the original name of zeolite, as given to this mineral by its discoverer Cronsted, in preference to that of mesotype given it by Haüy; and considers the distinction between mesotype and natrolith as unsupported by chemical analysis.

Feb. 14. A long paper on the effects of vegetable poisons on animals by Mr. Brodie was read. The author has pursued his researches for a considerable time, and detailed to the society the result of his experiments on rabbits, cats, and dogs, with alcohol, oil of bitter almonds, extract of aconite, tobacco, &c. These vegetable substances thrown into the stomachs of dogs, cats, rabbits, &c. instantly killed them by acting on the nervous system, and producing a compression of the brain: thrown into the rectum, the same effects were produced. The pulsation and heat of the heart, after administering these poisons, were maintained for a considerable time by means of artificial respiration, except with tobacco, which instantly destroyed the powers of the heart,

heart, and arrested the pulsations. One drop of the empyreumatic oil of tobacco let fall on the tongue of a cat killed her, but did not destroy the pulsation so instantaneously. Mr. Brodie made a great number of experiments with the vegetable poison used by the American Indians to poison their arrows, and with nearly similar results.

On Thursday, the 21st of February, a paper was read by H. Davy, Esq. LL.D. Sec. R.S. on a gaseous combination of oxymuriatic gas and oxygen.

Mr. Davy procured this extraordinary body by acting on hyperoxymuriate of potash by diluted muriatic acid. Mr. Davy stated, that it explodes by the application of a heat equal to that of the human body; and that though the oxygen and oxymuriatic gas expand in separating from each other, yet heat and light are produced. The metals which burn readily in oxymuriatic gas do not act upon this body till it is decomposed. He described a number of properties of this compound, all of which he considered as strengthening his opinion of the undecompounded nature of oxymuriatic gas; and as particularly opposed to the idea of its containing oxygen. Mr. Davy proposed the name zuthine or zuthic gas for this body from its colour, which is bright yellow; but he stated that he should be content to adopt any other name which might be considered as more appropriate.

ROYAL SOCIETY OF EDINBURGH.

On the 7th of January, Sir George Mackenzie continued his account of the mineralogy of Iceland, and described some very curious geological facts. On the 21st he concluded his mineralogical detail, with an interesting description of Mount Hecla, and other volcanic districts. In this paper Sir George made some remarks which tended to place obsidian and pumice in a conspicuous point of view, as relating to the different theories of the earth, and clearly proved their origin to be igneous; a position which has hitherto been denied by Werner and his pupils.

On the 4th of February, Dr. Brewster read an ingenious paper on the longitude of the comet of 1770. Sir George Mackenzie described some remarkable hot springs in Iceland. To one of these he gave the name of the *alternating Geyser*, as it spouted from two distinct orifices evidently connected within, but only from one at a time, whose operations alternated with those of the other, at regular intervals of time.

On the 18th, Professor Playfair read part of a biographical sketch of the late John Robison, LL. D. and Professor of Natural

Natural Philosophy in the University of Edinburgh.—Mr. Allan communicated a letter from Dr. Henry, of Manchester, describing the position of some singular masses of a substance apparently composed of wax and rosin, which had been laid bare by a late overflow of the river Mersey, a little below Stockport, about three feet under the soil,—and supposed to be the refuse of some manufactory, of which no other vestige or recollection now remains.

FRENCH NATIONAL INSTITUTE.

The following is a brief abstract of the Report of the National Institute at Paris for the year 1810.

Messrs. Gay-Lussac and Thenard have directed their attention to compare the relative powers and energies of different Galvanic piles. They have discovered that the force of the pile is not increased in proportion to the number of plates. To produce a double effect, the number of plates must be increased eight times. In general, it was found that the quantity of gas the piles will produce, is nearly in proportion to the cube root of the number of plates employed.—Amongst the discoveries to which the Galvanic pile has given rise, there are few more interesting to general chemistry than the transformation of the alkalies into combustible substances of metallic splendour.

This transformation, first discovered by Mr. Davy, was afterwards doubted by Messrs. Lussac and Thenard. In their former report they were disposed to consider potassium and sodium as combinations of the alkalies with hydrogen, and to class them amongst the compound substances called hydrurets: subsequent experiments have led them to incline to the opinion of Mr. Davy, and to regard potassium and sodium as simple metallic substances.

M. Berthollet has communicated a process for making the muriate of mercury, called mercurius dulcis or calomel, by passing oxygenated muriatic gas through mercury; it combines rapidly with the metal, and forms with it the muriate of mercury; and as this metallic salt has a perfect analogy with other mercurial salts produced by other acids and mercury at the minimum of oxidation, he concludes that the mercury, in forming this combination, has been reduced to an oxide by the oxygen of the acid, and not by that of the water.

M. Guyton has directed his attention to the mode of giving a permanent red colour to glass, by means of copper, which by accident he first discovered might be done.

M. Sage

M. Sage has also taken a part in these experiments, with the intent to colour glass red by means of copper and the phosphate of lime, or with bones; and he has shown crystals of glass, from the bottom of the pots used to melt glass in the bottle-manufacture at Seves, which had some resemblance to hexaëdral prisms.—It is well known that simple means have been discovered to extract soda from common salt. France formerly imported this article, so necessary to the arts: an inconvenience attended the mode of preparing it, from the quantity of acid gas which escaped, and was highly injurious. Amongst the different means of preventing this inconvenience which have been attempted, that of M. Pelletan the younger is deserving of notice. It consists in making the muriatic acid gas pass through long horizontal tubes partly filled with calcareous earth, which absorbs the gas, forming with it the muriate of lime. The experiments of M. Sage on plumbago (black lead) show that this substance does not contain any iron, but consists of a coaly matter mixed with one-tenth part of clay. The fossil carbon of St. Symphorien, near Lyons, approaches nearer to this substance than any other known mineral.

M. Deyeux has presented to the class of agriculture a loaf of sugar made from the red beet (*betterave*), which had all the whiteness and flavour of sugar from the cane. He has announced that this substance may be made in great quantities by the proprietors, who have devoted to this attempt 400 acres of ground. Should it succeed on the great scale, it will change the relations of the two worlds.

M. Dessaignes, principal of the college of Vendome, has continued his experiments on the different causes which produce luminous appearances; whether spontaneous, by friction, or gentle warmth, and in every other circumstance different from that of combustion. This philosopher obtained the prize on the same subject the last year: his present researches have considerably enlarged the results of his former experiments. In general, it has appeared to M. Dessaignes, that those bodies are the most phosphorescent that contain in their composition principles which can pass from a state of gas or liquid to a solid form. All bodies give out light by compression, whether they are fluid, solid, or gaseous. He has also discovered that points have the same effect on phosphorescence as upon the electric fluid. M. Dessaignes distinguishes two kinds of phosphorescence; the one transient, the other permanent. Amongst the first, we may state that which takes place when a certain quantity of water combines with quicklime; amongst the latter, that of rotten wood, and other organized substances in a

state

state of putrefaction. The latter more particularly occupied his attention. His experiments have been made on fresh-water fish, sea-fish, and various kinds of wood. The results of these experiments have proved, that the phosphorescence of each is a species of combustion, in which water and carbonic acid are produced. All the constituent parts of the animal muscle, and of wood, do not contribute to the light which is produced. The woody part, and muscular fibre, do not undergo in these changes any essential alteration. The phosphorescence of the wood is chiefly owing to a glutinous principle that serves to unite the woody fibres; and that of the flesh, to a gelatinous principle which unites the muscular fibres.

Messrs. Cuvier and Brongniart have discovered, in the environs of Paris, very extensive beds of stone, that contain only fresh-water shells, which appear to have been deposited in ponds or lakes. Some of these beds of stone are separated by intermediate banks of marine formation. This seems to prove that the sea has made an irruption on the continent which it had formerly abandoned, and confirms the traditions of a deluge so universally spread amongst different nations.—Upon beds of gypsum in the same neighbourhood, which contain the bones of reptiles, and of fish, with fresh-water shells, and petrified trunks of palm trees, repose other beds of stone, containing innumerable quantities of marine shells only; and again upon these, other beds of fresh-water shells, but of a kind entirely different from the former.—It is impossible to have more clear and manifest indications of the revolutions which have taken place on the surface of the globe.

M. Sage and M. Cubieres have directed the attention of philosophers to a singular fact, which has excited innumerable conjectures. In the neighbourhood of Puzzoli, three erect columns of a small temple have been discovered, thirty French feet below the present level of the sea, all pierced and bored to the same height, by dails and polades, a kind of marine shell-fish, which penetrate into the densest stones immerged under the surface of the sea. — Have these columns been taken from a quarry formerly under the surface of the sea? — But why should they have chosen stones so perforated? and how does it happen that the perforations extend exactly to the same height in each column?—Has the temple been successively sunk and raised again in a volcanic country subject to so many irregular movements?—But how, after such violent shocks, could the columns remain erect?—Have volcanic eruptions opened deep ravines which have closed at one extremity,

tremity, and have kept the temple confined in a kind of lake until a passage was opened, and restored the ground to its natural dryness?—There are difficulties attending all these explanations, particularly the two last. How could such important changes have taken place after the construction of the temple, and have left no trace in history or in the memory of man? They frequently speak of the eruption in the year 1528, when the hill called *Monte-Nuovo* was formed, and when the sea invaded a part of the coast; but there is no tradition of successive revolutions.—Near this temple has been discovered a particular variety of marble, of which M. Cubieres has read an analysis to the Institute. It is white, semi-transparent, and receives a fine polish; it dissolves with difficulty in the nitric acid, and gives sparks with steel; it contains 22 parts in every hundred of magnesia. M. Cubieres has called it the *Greek Magnesian Marble*, and thinks it the same which the ancients made use of to construct their temples without windows, that received light only through the transparent walls.—M. Daubisson has given an account of a mine of lead near Tranowitz in Silesia, containing the ore called galena in a very extensive bed. It is found in rocks composed of shells, which Mr. D. thinks are of recent formation. To know really the age of the calcareous beds in which the lead ore is found, we should determine the species of shells that they contain.

XXIX. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir, Having observed in a late publication of the first respectability, that the writer of the article on "Freezing Mixtures" regrets that I have not given the specific gravities of the *acids* used in my experiments; I beg leave to state, for the information of that gentleman, and others whom it may concern, that the specific gravity of the *sulphuric acid* was 1·848; and that of the *red fuming nitrous acid* 1·510; both of which are thus given in a paper by me in the Philosophical Transactions for 1795; and in my Treatise on Artificial Cold, page 76.

I take this opportunity of declaring my intention of presenting, through the medium of the Philosophical Magazine, some observations relating to the practice of Physic and Surgery, collected by me, during a residence of nearly five-and-twenty years in the Radcliffe Infirmary, as apothecary to that institution.

In

In the course of the period above mentioned, I had an opportunity of attending to, and watching, nearly all the cases chirurgical as well as medical which occurred, amounting to upwards of *nineteen thousand* patients; making a journal occasionally, which I have by me, consisting of brief remarks respecting the progress and event of the various diseases which presented, and the effects of the different medicines and applications administered in those diseases.

I have adopted this method of communicating what I may wish to say on this subject, the whole of which will be comprised in a small compass, in consequence of my present occupations allowing me scarcely any leisure, even for the purpose of epitomizing the result of my experience.

My chief object therefore will be, to point out, under the form of a *compendious classification*, the diseases which are *curable*, and the most appropriate or efficacious remedies for that purpose; and likewise the means of palliating the effects of such diseases as I consider from experience to be *incurable*.

It must be obvious, that out of such a great number of patients, almost every species of disease, as well as a *very frequent recurrence of each*, must have passed under my notice: hence the discrimination of diseases, and the respective merits of all the medicines and applications then in use, cannot but be extremely familiar to me.

I am, sir,
Your obedient servant,
RD. WALKER.

Oxford, Feb. 18, 1811.

P. S.—In the course of my papers, I shall have occasion to mention some small improvements, as I hope, in the practice of surgery, &c. made by myself.

To Mr. Tilloch.

SIR,—MR. FAREY'S observation on affixing new and separate ideas to established marks or words, is perfectly correct; and had it not been for an error of the printer, he would not have had occasion for the remark; as, by referring to the letter I had the honour of addressing you, it will be seen that the grave accent (`) and not the acute accent (′) was intended, as the same objection occurred to me at the time. As the form of the character is of no consequence if the idea is approved, the printers can have recourse to any *new one* which may suit them, only observing not to alter it when once adopted.

I remain with great respect, sir,
Your obliged humble servant,
A. REIRTALP.

Spitalfields, Feb. 11, 1811.

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For January 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 27	52	46°	43°	29·62	20	Fair
28	40	38	32	30·18	18	Fair
29	31	36	31	·35	21	Fair
30	30	34	29	·52	17	Fair
31	29	31	29	·45	10	Fair
Jan. 1	28	28	27	·20	0	Snow
2	28	29	26	29·93	6	Cloudy
3	25	24	21	·68	0	Snow
4	28	29	25	·85	0	Cloudy
5	25	28	26	·76	10	Fair
6	28	29	25	·80	9	Fair
7	27	28	27	·82	10	Fair
8	27	28	26	·76	9	Cloudy
9	26	29	29	·82	0	Snow
10	24	30	35	·82	0	Foggy
11	44	43	41	·70	0	Rain
12	42	45	40	·40	0	Rain
13	35	43	43	·62	16	Showery
14	46	48	42	·63	18	Fair
15	46	45	35	·65	21	Fair
16	33	42	43	·78	0	Showery
17	45	47	45	·61	0	Rain
18	41	40	35	·70	5	Cloudy
19	33	40	34	30·30	15	Fair
20	32	39	33	·29	10	Fair
21	34	41	34	·01	0	Rain
22	34	38	33	·16	0	Foggy
23	35	40	39	·20	0	Rain
24	35	41	35	·40	15	Fair
25	35	37	30	·45	9	Cloudy
26	28	39	38	29·99	5	Cloudy

N.B. The Barometer's height is taken at one o'clock.

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For February 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	38	40	33	29·58	10	Fair.
28	28	33	28	·59	5	Fair.
29	24	29	24	·60	7	Fair.
30	21	32	30	·62	0	Cloudy, great fall of snow during the night.
31	34	42	39	·05	0	Rain.
Feb. 1	38	42	41	·30	0	Rain.
2	44	46	40	·40	5	Cloudy.
3	44	46	36	·45	7	Cloudy.
4	32	45	37	30·05	21	Fair.
5	36	39	39	29·95	10	Cloudy.
6	44	49	45	·52	12	Cloudy.
7	42	47	44	·54	25	Fair.
8	45	48	45	·52	18	Stormy.
9	40	46	43	·65	10	Cloudy.
10	45	50	50	·60	0	Small Rain.
11	50	50	49	·25	0	Stormy.
12	49	49	38	·11	0	Rain.
13	35	40	35	·20	0	Stormy.
14	37	42	40	·40	17	Fair.
15	40	42	35	·51	15	Cloudy.
16	36	39	33	·80	22	Cloudy.
17	29	35	37	30·20	10	Cloudy.
18	37	43	35	·02	21	Cloudy.
19	35	42	34	29·82	19	Fair.
20	33	41	38	·66	16	Cloudy.
21	39	43	44	·09	0	Small Rain.
22	42	51	44	·01	33	Fair.
23	41	49	45	·05	35	Fair.
24	44	55	54	·01	0	Rain.
25	41	49	44	·25	32	Fair.

N. B. The Barometer's height is taken at one o'clock.

XXX. *A List of about* 700 Hills *and Eminences in and near to Derbyshire, with the Stratum which occupies the Top of each, and other Particulars,—and the Answers received, to Inquiries in our last Volume, respecting Mr.* Michell *and Mr.* Tofield's *Geological Manuscripts, &c. By Mr.* John Farey, Sen., *Mineralogical Surveyor.*

To Mr. Tilloch.

Sir, In my Report on the Agriculture and Minerals of *Derbyshire,* the first volume of which, treating of the surface, minerals, rivers, &c. is now in the press, An alphabetical List of the most remarkable Mountains, *Hills* and Eminences has been printed, referring to a small Map of 41 principal Ridges, or ranges of high ground, called water-heads, which divide the drainages to 32 rivers and rivulets, in and near Derbyshire. By a series of numbers, placed on or near to each ridge in the Map, the situation of the several hills, peaks, tors, cliffs, banks, edges, tops, heads, stones, lows, or other highest or otherwise remarkable and elevated points of the surface, are pointed out; and their situation, with respect to the Towns, is mentioned, and the stratum on the top of each hill in the List; the mines, quarries, caverns, &c. on them are also noticed, in some instances. At the conclusion of the printing of this part of my Report, it occurred to me, that a List of the Hills and their upper strata, in the order in which they are numbered on the several ridges, would be not less useful, than the alphabetical one above mentioned, particularly to Residents, and to Geological Travellers when on the spot, on which account I have sent you such a List, in case you deem it worthy of a place in your Philosophical and Geological Magazine, and in which case, you may perhaps, at a future time, give a copy of the *Map* of Ridges, on which these numbers are found.

It may be proper here to remark, that I found it necessary, in three instances, to deviate from the plan of naming the Ridges by the river-districts, or drainages which they bound, viz. the *Grand Ridge* extending from near Saddleworth on the Huddersfield Canal in Yorkshire, to near Harecastle on the Trent and Mersey Canal in Staffordshire: the *Ashby and Burton* Ridge, extending nearly from one of these towns to the other in Leicestershire and Staffordshire, across an angle or corner of Derbyshire; and the *Charnwood* Ridge, extending from Mount Sorrel to Thringston,

ton, across Charnwood Forest in Leicestershire. Such Hills as have only a number prefixed, are situated *on* or very near to the Ridge or Waterhead, and in the other cases, the letters E, W, N, or S, point out, the side of the ridge on which they are situated, which is never beyond the River whence the ridge is named: and such collateral Hills, it is to be observed, and the branch Ridges on which they are situate, will sometimes be found as high or even higher, than the principal Ridges. I shall, as I have before said, be thankful for the additions and corrections of your readers and correspondents, respecting these particulars, and am,

Sir,

Your obedient servant,

Westminster, March 4, 1811. JOHN FAREY, Sen.

South AMBER *Ridge*, extending from Greenwich Village (No. 60), on East Derwent Ridge, nearly W. 5 Miles. (Green).

No.		No.	
1	Ripley Village, Coal Shale.	3 S.	Belper Windmill-hill, 2d Grit.
2	Stone Hill, 3d Grit, high.	4	Toadman Hill, 2d Grit.

West AMBER *Ridge*, extending from Hartwood Hill (No. 36) on East Derwent Ridge, S.S.E. about 11 Miles (Lake red).

No.		No.	
1	Ashover-quarry Hill, 2d Grit, grindstones.	12 E.	Ogstone Hill 4th Grit.
2	Wirestone Hill, 2d Grit.	13 E.	Washington Green (Wessington), 3d Coal Shale.
3 W.	Haredge Hill, 2d Grit, high.	14	Lindow-Lane Hill, a hummock of 2d Grit.
4	Overton Park, 1st Grit, very high.	15	Wheatcroft village, 1st Coal Shale.
5	Slag Hills, 2d Grit, very high.	16 W.	Upper Holloway, 1st Grit.
6 W.	Riber Top, 1st Grit, (see Section in Plate II. in vol. xxxi.)	17	Crich Cliff (monument), 1st Lime, very high.
7 W.	High Tor, 1st Lime. (do.)	18 E.	Park-Lane Head, 3d Grit.
8 W.	High Lees, 1st Grit, very high.	19 E.	Coburn Hill, 4th Grit, Quarry.
9 W.	Castle-top, Shale & Shale Grit.	20 E	Fritchley Hill, 1st Grit.
10 E.	High Oredish, 1st Grit.	21	Crich Chase, 1st Grit.
11 E.	Hay Hill, 1st Grit.		

ASHBY and BURTON *Ridge*, extending from Butt House (No. 13), and Wooden-Box Bar (No. 16) on North Mease Ridge, E.S.E. and W.N.W. 8 Miles in length (Blue.)

No.		No.	
1	Ashby Hill, Gravel on red Clay.	8 N.	Repton Hill, Red Marl.
2 S.	Willesley Hill, red Clay.	9 N.	Bladon Hill, Gravel on Red Marl.
3 S.	Odd-house Hill, red Clay.	10 S.	Brislingcote Hill, Red Marl. (not No. 60.)
4	Smithsby Windmill-hill, red Clay.	11 S.	Stanton Hill, Gravel.
5	Midway Houses, red Clay.	12	Scropley Hill and Clump, Red Marl.
6 S.	Newhall Windmill-hill, Grit.		
7 N.	Bretby Clump, Red Marl.		

East

East ASHOP *Ridge*, extending from Blakelow Stones (No. 9) on Grand Ridge, nearly S.E. 9 Miles. (Orange.)

No.
1 Alpert-Castles Hill, Shale Grit, large Slips.
2 Rowlee Peat-pits, Shale Grit.
3 Crook Hills, two Hummocks of 1st Grit, on Shale.

West BOOTLE *Ridge*, extending from Ripley Village (No. 1) on South Amber Ridge, S.S.W. about 7 Miles. (Vermilion.)

No.
1 Henmore, 4th Grit.
2 High Wood, 3d Grit.
3 Holbrook Moor, 3d Grit.
4 Duffield Bank, 1st Grit.

East BRADFORD and LATHKIL *Ridge*, extending from Mining Low (No. 61) on West Derwent Ridge, N.E. about 6 Miles. (Orange.)

No.
1 Blakelow (near Elton) 1st Lime.
2 E. Islington Hill, 2d Lime.
3 E. Grey Tor (or Gree), 1st Lime Hummock.
4 E. Bank-pasture Tor, 1st Lime Hummock.
5 E. Blakelow (near Bright Gate, 2d Lime, very high.
6 E. White Low, 3d Lime.
7 E. Wensley Village, Shale and 1st Lime.
8 E Oaker Hill, Shale & Shale Grit, slips.
9 E. Masson Low, 2d Toadstone, very high; (see Section in Plate II. vol. xxxi.)
10 E. Heights of Abraham, 1st Lime and 1st Toadstone.
11 W. Berry Cliff, 1st Grit.
12 W. Hartle Moor, 1st Grit.
13 W. Mock-Beggar Hall (or Robin Hood's Stride), 1st Grit? Hummock.
14 Stanton Moor, 1st Grit Hummock
15 W. Rowter Rocks, 1st Grit? Hummock.
16 W. Stoney-Lee Rocks, Shale Grit, coarse.

CHARNWOOD *Ridge* in Leicestershire, extending from Mount Sorrel to Thringston, touching West Soar Ridge between Cliff and Bardon Hills (No. 6 and 7), E. and W. about 14 Miles. (Green).

No.
1 Castle Hill, Sienite, Quarries, for Paving.
2 Mount Sorrel Windmill-hill, Sienite.
3 S. Scampton Hill, Sienite.
4 N. Hawksley Hill, Sienite.
5 Longclay Hill, Sienite.
6 N. Beddow Wood, isolated, large and high, Sienite.
7 N. Kinsley Hill, Sienite.
8 Rothley Hill, Quartz Gravel, on Red Marl.
9 Cropstone Village, Leicestershire, Red Marl.
10 Stewards Hay, coarse Slate, with Sienite S. of it.
11 S. Martin-shaw Wood, coarse Slate.
12 S. Grooby Town, Leicestershire, Sienite Quarries, for Paving.
13 Markfield Toll-Bar, coarse Slate
14 S. Markfield Windmill-hill, high, conical, Sienite.
16 Copt Oak, (also on West Soar Ridge,) coarse Slate.
17 N. Hamer Cliff, coarse Slate.
18 N. Stoney-well Hill, coarse Slate.
19 N. Baldwins-castle Hill, coarse Slate.
20 N. Bens Cliff, coarse Slate, one of the highest Peaks in Charnwood Forest.

No.
21 N. Crow Hill, coarse Slate.
22 N. Old John Hill, coarse Slate.
23 N. Bradgate Park, West Hill, part Sienite and part coarse Slate
24 N. Newton Linford Hill, part Sienite and part coarse Slate.
25 N. Bradgate Park, S.E. Hill, coarse Slate.
26 N. Hunters Hill, coarse Slate.
27 N. Swithland Wood, coarse Slate, large ancient Quarries.
28 N. Broombridge Hill, coarse Slate.
29 N. Brand Hill, coarse Slate, the Swithland Slate Quarries.
30 N. Bassil Wood, Sienite, with Pyritic Veins?
31 N. Beacon Hill, coarse Slate, perhaps the highest Hill in Charnwood Forest.
32 N. Bird-(or Windmill-)hill, coarse Slate, very high.
33 N. Black bird's-nest Hill, coarse Slate.
34 N. Hanging-stone Hill, coarse Slate.
35 N. Outwoods, coarse Slate.
36 N. Whittle Hill, Gravel, and Slate, Whetstone Quarries.
37 N. Goathouse Hill, coarse Slate.
38 N. Five-dale Trees, coarse Slate.
39 N. Charley Knowl, coarse Slate.
40 N. Lubs-Cloud Hill, coarse Slate.
41 N. Iveshead Hill, coarse Slate.
42 N. Morley Hill, coarse Slate.
43 N. Gibbet Hill, coarse Slate.
44 N. Finney Hill, coarse Slate.
45 N. Broadhurst Hill, coarse Slate, (not Nº 46.)
46 N. Garendon Park, Red Marl.
47 Birchwood Hill, coarse Slate.
48 S. Irish Hill, coarse Slate.
49 S. Timberwood Hill, coarse Slate.
50 Warren Hill, coarse Slate.
51 Long Hill, coarse Slate.
52 N. Tin Meadow Hill, coarse Slate.
53 N Flat Hill, coarse Slate.
54 N. Sharp Hill, coarse Slate.
55 Houghton Hill, coarse Slate.
56 Cademan Hill, coarse Slate.
57 Broad Hill, coarse Slate.
58 Cinder Hills, coarse Slate, red.
59 N. Grace Dieu House, red Grit? on Limestone
60 N. Grace Dieu Toll-Bar, red Grit?

East CHURNET *Ridge*, extending from Brown Middle Hill (No. 33), on Grand Ridge, S.S.E. 18 Miles. (Yellow.)

No.
1 Black-meer Hill, Shale, Slipt at the Meer.
2 Moor-cock Hall, Shale and Shale Grit.
3 E. Mixon Hill, Shale Limestone, and Copper Mines.
4 W. Leek Common, Shale and Shale Grit.
5 Moredge Hill, Shale and Shale Grit.
6 Ipstone Edge, 1st Grit & Shale.
7 W. Sharp Cliff, Gravel Rock? on 1st Grit.
8 W. Ferney Hill, Quartz Gravel and Shale.
9 W. Yew Tree Hill, Quartz Gravel.
10 W. Belmont Hill, 2d Grit.
11 Pike Low, 1st Grit.
12 W. Old Ridge, 2d Grit.
13 W. Crow Trees, 2d Grit.
14 W. Three Lows, Shale and Shale Grit.
15 W. Bee Low, a 2d Grit Hummock.
16 Caldon Low, 4th Lime, large Quarries.
17 E. Musden Low, 4th Lime.
18 E. Hunters Knowl, 4th Lime.
19 E. Swincote Hill, Shale Limestone.
20 W. Beacon Hill, 4th Lime, high.
21 Weaver Hill, 4th Lime, very high, a Station in the Government Trigon. Survey.
22 Cliff House, Quartz Gravel on Red Marl.

West CHURNET and DOVE *Ridge*, extending from Biddulph Moor Hill (No. 38), on the Grand Ridge, S.E. 38 Miles. (Yellow.)

No.
1 Brown-edge coarse Grit Rocks of the upper Coal series.
2 Tenters Wood Hill, coarse Grit Rocks, do.
3 Wetley Moor.
4 Cellar Head, Gravel, on Marl and Limestone beds
5 E. Wetley Rocks, 1st Grit.
6 E. Heath House, Shale.
7 E. Kingsley Moor, 2d Coal Shale.

8 E. Cheadle

No.
8 E. Cheadle Park, Quartz Gravel.
9 E. Hazlecross Hill, 2d Grit.
10 E. High-shut Hill. Quartz Gravel.
11 E. Tithe Barn Hill, Red Marl.
12 E. Rocester Common, Red Marl.
13 E. Hollington Heath, Quartz Gravel.
14 Bank Top, 1st Grit and Shale? with Marl and Limestone Beds.
15 Dilhorn Hills, Quartz Gravel.
16 Loxley Green, Quartz Gravel, on Red Marl.
17 E. Hollington Hill, Quartz Gravel, on Red Marl.
18 Bagot Park, Red Marl.
19 W. Newborough Windmill-hill, loamy Gravel, on Red Marl.

No.
20 Marchinton Cliff, Shale on Red Marl.
21 E. Hound Hill, Red Marl, and Gypsum Quarries.
22 Six Hands, loamy Gravel, on Red Marl (not Seven).
23 E. Hanbury, Red Marl.
24 E. Fauld Hill, Red Marl, and Gypsum Quarries.
25 E. Tutbury Castle Hill, Red Marl.
26 W. Christ Church Hill, loamy Gravel, on Red Marl.
27 Callengwood Hill, loamy Gravel, on Red Marl, high.
28 W. Wichnor Park, Red Marl.
29 W. Sinai Park, Red Marl.
30 W. Beacon Hill, Red Marl.

North DANE *Ridge*, from Anders Edge (No. 3), on West Goyte Ridge, extends beyond the limits of the Map. (Blue.)

No.
1 Dimpas Hill, Shale and Shale Grit.
2 N. Nabs Nose, 4th Grit, reddish Grey Slate Pit, high.
3 N. Cliff Bank, 3d Grit.
4 N. Blakelow Hill, 4th Grit.
5 N. Kerredge, 4th Grit, large Quarries.

No.
6 Shutlings Low, a Hummock of 1st Grit, high, & remarkable.
7 Homerton Nose, 1st Grit.
8 S. Mins Hill, Shale & Shale Grit.
9 Stoney-fold Hill, Shale and Shale Grit.
10 Macclesfield Town, Cheshire, Quartz Gravel, on Red Marl.

East DERWENT *Ridge*, extending from Dean-head Stones (No. 6), on Grand Ridge, nearly S.E. about 67 Miles. (Blue.)

No.
1 Horse Stones, 1st Grit.
2 Cut Gate, deep Peat, on 1st Grit.
3 E. Pike Low, 1st Grit.
4 High Stones, 1st Grit.
5 Holden Moor, 1st Grit.
6 E. Broomhead Moor, Peat, on 1st Grit.
7 E. Bradfield Moor, 1st Grit.
8 E. Hell-field Moor, 1st Grit.
9 Lost Lad, 1st Grit.
10 Dovestone Tor, 1st Grit.
11 Stake Hill, 2d Grit, a Gamekeeper's house on it.
12. Crow Chine, 1st Grit.
13 W. Derwent Edge, 1st Grit, very high.
14 W. Bamford Edge, 1st Grit.
15 Stanage Hill, 1st Grit.
16 E. Lords Seat, 2d Grit.
17 Stanage Pole, 2d Grit.

No.
18 E. Rud Hill, 2d Grit.
19 E. Hallam Post Hill, 1st Grit.
20 E. Walkley Bank, 3d Grit.
21 W. Over Stones, 1st Grit.
22 W. Higger Tor, a Hummock of 1st Grit.
23 W. Scraper Low, Shale and Shale Grit.
24 W. Old Booth Edge, 1st Grit, the famous Peak Mill-stone Quarry, (not on Ridge.)
25 Ox Stones, 2d Grit?
26 Hathersage Ridge, 2d Grit, (a Station in the Government Trigonometrical Survey.)
27 Shepherd's Moss-House Hill, 2d Grit.
29 Gorsey Bank, 1st Grit.
31 E. Grange-hill Top, 2d Grit.
32 E. Bole Hill, 8th Grit.
33 Pudding pie Hill, 4th Grit.

No.
35 W. Bunkers Hill, 3d Grit.
36 Harwood Hill, 3d Grit.
37 E. Holy-moor Top, 3d Grit.
38 W. Blakelow, 2d Grit, high.
39 Roches Rock, 1st Grit, famous Fire-stone Quarry.
40 AliseHead, 2d Grit. (N° omitted.)
41 Spite-winter Hill, 2d Grit.
42 E. Stanage Hill, 3d Grit, 3 clumps of trees on it.
43 E. Boythorpe Hill, 10th Grit.
44 E. Wingerworth Park, 10th Grit.
45 W. Fabric Hill, 1st Grit.
46 W. Deer-leap Hill, 4th Grit.
47 Birkin Lane, 3d Coal Shale, good Fire Clay.
48 Bole Hill, 4th Grit, Quarries, clothed with Fir Woods.
49 Stretton Village, 10th Grit.
50 Harstoft Common, Grit.
51 Cock Top, Grit.
52 Over Moor, Coal Shale.
53 Hucknal Windmill-hill, yellow Lime.
54 Sutton Hill, yellow Lime.

No.
55 Normanton Common, Coal Shale.
56 Cotes Park, Coal Shale.
57 Somercoats Green, Coal Shale.
58 W. Swanwick Windmill-hill, Coal Shale.
59 W. Castle Hill, 7th Grit.
60 Greenwich, Coal Shale.
61 E. Codnor Park, 10th Grit.
62 Breach Hill, Grit.
63 Roby Field, Grit.
64 E. Shipley Park, Grit.
65 E. Park-Hall.
66 W. Bredsall Moor, Salmon-coloured Grit.
67 Chaddesden Moor, alluv. Clay.
68 E. Hag Windmill-hill, Salmon-coloured Grit.
69 Dums Hill, Quartz Gravel.
70 W. Spondon Hill, Red Marl.
71 E. Clouds Hill, Red Marl and Gravel.
72 E. Risley Windmill-hill, Red Marl.
73 Risley Park, Red Marl, high.
74 Hopwell Hall, Red Marl.

West Derwent *Ridge*, extending from Axe-edge Hill (Middle, No. 31), on the Grand Ridge, nearly S.E. about 46 Miles. (Green.)

No.
1 Thirkelow, 4th Lime.
2 W. High Edge, 4th Lime.
4 Harper Hill, 4th Lime.
5 E. Fox Low, 4th Lime.
6 E. Staden Hill, 3d Toadstone.
7 E. High Cliff, 4th Lime.
9 E. Hill Head, 4th Lime.
10 High Low, 4th Lime.
12 W. Croom Hill, 4th Lime, sharp and wedge like.
13 W. Park-house Hill, 4th Lime.
14 Brierly Hill, 4th Lime.
15 Dow Low, 4th Lime.
16 W. Alders Cliff, 4th Lime.
17 W. High Wheeldon, 4th Lime.
18 W. Cronkstone Hill, 3d Lime.
19 W. Hurdlow Hill, 4th Lime.
20 Great Low, 4th Lime.
22 Over Street, 3d Lime.
23 Hurdlow-house Hill, 2d Lime.
25 W. Waggon Lows, 4th Lime.
26 W. High Cross, 4th Lime.
27 W. Cliff Hill, 4th Lime.
28 W. Mossey Low, 4th Lime.
30 W. Cardel Low, 4th Lime.
31 Coatsfield Low, 3d Lime.
32 Benty-Grange Hill, 2d Lime.
33 E. One Ash Clump, 1st Lime.

No.
35 W. Lean Low, 4th Lime.
36 W. Castern Low, 4th Lime, with a clay Wayboard and Well near the top.
37 W. Hen Low, 4th Lime.
38 W. Bullock Round, 4th Lime.
39 W. Pine Low, 4th Lime.
40 Wolfscote Hill, 4th Lime.
47 W. Cross-low Bank, 4th Lime.
48 W. Mote-low Arbor, 4th Lime, high.
49 W. Nabs Hill, 4th Lime.
51 W. Thorpe Cloud, 4th Lime, sharp and wedge like.
52 E. Arbor Low, chert Rubble on 2d Lime, very high, a Druidical circle of stones on it.
53 E. Callenge-low Hill, 1st Lime.
55 E. End Low, 1st Lime.
58 E. Gratton Low, 1st Lime.
59 Elklow (or Hillock Low), 4th Lime, very high.
60 Gotam Hill, 4th Lime.
61 E. Minning Low, 4th Lime.
62 E. Gallows Low, 4th Lime.
63 E. Great Edges, 3d Lime.
64 E. Little Edges, 3d Lime.
65 E. Sharrat Cliff (or Shadycliff), 3d Lime.

66 E. Ald-

No.
66 E. Aldwark Hill, a Hummock of 3d Lime. (N° omitted.)
67 E. Moot Low, 4th Lime.
68 Elder Tor, 4th Lime.
69 W. Peters Pike, 4th Lime.
70 W. Hoe Cliff, 4th Lime.
71 W. Ipley Hill, and Clump, 4th Lime.
72 W. Reynards Tor, 4th Lime.
73 Ryda Hill, 4th Lime
74 Harboro Rocks, 3d Toadstone or Dunstone, very high, a Well and Hermitage near the top.
75 E. Charriot Clump, Chert, &c. Rubble on 3d Toadstone.
76 Barn Hill (Wethericks), 3d Toadstone, (not Lime.)
77 W. Hascar Hill, Shale and Shale Grit.
78 Copt Holly Hill, Shale and Shale Grit.
79 Blackwall Hill, a hummock of Quartz Gravel, on Shale.
80 E. Cliff Ash Hill, a hummock of Quartz Gravel, on Shale.
81 Gib Hill, Shale.
82 Wardgate Hill, Gravel.
83 Hulland Ward Village, Shale.
84 Derby Hill, Quartz Gravel, on Shale.
85 Mansell Park, Shale, and Gravel on Red Marl, the Great Fault crosses it.
86 Brailsford Hill, Gravel, on Marl.
87 Snaper Hill, Red Marl.
88 E. Priestwood Hill, Red Marl.
89 Rough Heanor, Red Marl.
90 W. Radburne Hall, Red marl.
91 W. MickleoverTown, Red Marl.
92 W. Burnaston Hill, Red Marl.
93 W. ConeygreeHill, loamy Gravel, on Red Marl, (not N° 92.)
94 Littleover Town, Gravel, on Red Marl.
95 W. Stenson Hill, Gravel, on Red Marl.
96 W. Swarkestone Lows, Gravel, on Red Marl.
97 E. Peters Hill, Gravel, on Red Marl.
98 Normanton Hill, Red Marl.
100 Chellaston Hill, alluvial Clay, on Gypsum and Red Marl: Gypsum Quarries.
101 W. Wymans Hill, Red Marl, and Gypsum.
102 Weston Cliff, Red Marl, and Freestone Quarries, by the Grand Trunk Canal.
103 E. Ballington Hill, Red Marl and Gypsum.

West DOLEE *Ridge*, extending from Harstoft Common (No. 50), on East Derwent Ridge, N. about 8 Miles. (Green.)

No.
1 Compton Common, Grit.
2 W. Temple Normanton Town, 13th Grit.
3 Heath Hill, 13th Grit; (not 12th, East.)
4 Sutton Hall, 13th Grit.
5 Bole Hill, 9th Grit.
6 W. Upper Lane Farm, 5th Coal Shale, denudated.
7 W. Brimmington Town, 8th Grit.
8 Inkersall, 13th Grit.
9 Hawthorn Hill, 12th Coal Shale.

East DOVE *Ridge*, extending from Snaper Hill (No. 87), on the West Derwent Ridge, nearly S. about 10 Miles. (Lake.)

No.
1 Longlane Hill, Red Marl.
2 Cropo Top, Red Marl.
3 Ash, Red Marl.

East ECCLESBURN *Ridge*, extending from Barn Hill (Wethericks, No. 76), on West Derwent Ridge, S.S.E. about 11 Miles. (Yellow.)

No.
1 Middleton Intake, 3d Lime.
2 Middleton Moor, 3d Lime.
3 E. Dunrake Hill, 1st Lime; (not No. 2.)

No.

4 Middle Peak, 3d Lime.

5 Barehill Edge, 1st Grit, very high.

6 E. Stone-house Rocks, a 1st Grit Hummock.

7 E. Longway Bank, 2d Grit, and 1st Coal Shale.

8 E. Alderwasley Windmill-hill, 2d Grit.

9 Alport Hill, 1st Grit, very high, a Station (Orpit.) in the Government Trigonometrical Survey.

No.

10 Coneygree Hill, 1st Grit.

11 E. Chevin N. Hill, 1st Grit.

12 E. Mount Pleasant, 2d Grit.

13 Alton Hill, Shale and Shale Grit.

14 W. Hill Top, Shale and Shale Grit.

15 Shottle Gate Hill, Shale.

16 E. Far Low, 1st Grit.

17 Chevin Hill, 1st Grit, high.

18 W. Hazlewood Hill, Shale.

19 Milford Bank, 1st Grit.

East EREWASH *Ridge*, extending from Cocksmoor-Clump (or Robin Hood Hills, No. 1), on South Idle Ridge, S. 15 Miles. (Yellow.)

No.

1 Pearl Hill, Quartz Gravel, on yellow Lime.

2 Annesley Hill, Quartz Gravel.

3 W. Selstone Park.

4 W. Sheepston Hill, a hummock of Gravel, on Coal Shale, (not on Ridge.)

5 W. Underwood Village, North. 14th Grit, (not 4th.)

6 W. Brinsley Windmill-hill, Grit.

7 W. Greasley Hill, yellow Lime.

8 W. Eastwood Town, Notts.

No.

10 Strelly Windmill-hill, yellow Lime, on Coal Measures.

12 Wollaton Hall, Gravel, on Coal Shale.

13 Trowel Moor, Coal Shale.

14 W. Bramcote Hill Quartz Gravel, on Coal Shale.

15 Bramcote Town, (Notts.) Quartz, Gravel on Red Marl.

16 E. Beeston Windmill-hill, Quartz Gravel on Red Marl.

North ETHROW *Ridge*, extending from Holm Moss (No. 1), on the Grand Ridge, S.W. about 11 Miles. (Green.)

No.

1 Goodgrave Edge, Peat on 2d Grit, very high.

2 N. Dove-stones Moss.

3 Wall-stone Edge, 1st Grit.

4 N. Offin Stone, 1st Grit.

5 N. Abraham Chair, 1st Grit.

6 N. Buckton Castle Hill, 1st Grit.

7 N. Harridge Pike.

No.

8 Wild Bank, 3d Grit.

9 N. Hough Hill, 3d Grit.

10 S. Mottram Hill, a hummock of 4th Grit.

11 Harrop Edge, 3d Grit.

12 Werneth Low, alluvial reddish Marl, on 3d Grit.

West GOYTE *Ridge*, extending from Thatch Marsh (No. 28), on the Grand Ridge, N.N.W. about 15 Miles. (Yellow.)

No.

1 Stake Top, Peat on 2d Grit, very high.

2 Goyteshead Tor, 1st Grit, very high.

3 Anders Edge, Shale and Shale Grit.

5 W. Rainow Low, 4th Grit.

No.

7 Bowstone Cross, 1st Grit.

8 Cocks Knowl, Shale and Shale Grit.

9 Whaley Moor, 2d Grit, high.

10 Jackson's Edge, 3d Grit.

11 Marple Chaple, 3d Grit.

The

The Grand Ridge, extending from near Saddleworth in Yorkshire, to near Talk in Staffordshire, S.W. nearly 66 Miles, within the limits of the Map. (Vermilion.)

No

1 Holm Moss, deep Peat, on 2d Grit.
2 Bretland Edge, Peat, on 2d Grit.
3 Dean Edge, Peat, on 2d Grit.
4 Withins Mouth, Peat, on 2d Grit.
5 Ladycross Hill, 2d Grit.
6 Dean-head Stones, 1st Grit.
7 E. Barrow Stones, 1st Grit.
8 E. Grinah Stones, 1st Grit.
9 Blakelow Stones, 1st Grit, the highest Hill in Derbyshire.
10 Wain Stones, 1st Grit.
11 Shelf Stones, 1st Grit.
12 Alport Low, a hummock or isolated patch of 1st Grit.
13 Old Woman, a heap of sods of Peat, upon Shale.
14 Glead Hill, 1st Grit.
15 W. Whimbury Knot, 1st Grit.
16, 17 Kinder Scout Hills, 1st Grit, the 2d in height in Derbysh.
18 Edale Head, 1st Grit.
19 W. South-head Tor, 1st Grit.
20 W. Chinley Churn, 2d Grit.
21 Rushop Edge, Shale and Shale Grit.
22 Barmoor Hill, 4th Lime.
23 E. Kems Hill, 4th Lime.
24 { E. Bole-end Hill, a hummock of 2d Lime.
E. Heathy Low, 2d Toadstone.
E. Nabs Buts, a 2d Lime hummock. }

No.

25 E. Knot Low, a hummock of 2d Toadstone.
26 Ridge Coit, 3d Lime.
27 Combs Moss, a large hummock of 1st Grit, with 1st Coal Shale on it.
28 Thatch Marsh, 2d Grit.
29 Axe-edge Hill (great or North), Shale and Shale Grit, very high, 625 yards above the Sea.
30 E. Grin Hill, 4th Lime, Lime Kilns, Pools hole Cavern.
31 Axe-edge Hill (middle), 1st Grit.
32 Axe edge Hill (South), 1st Grit.
33 Brown Middle Hill, Shale and Shale Grit, (not N° 83.)
34 Ramshaw Rocks, 1st Grit.
35 High Roches, 1st Grit, very high.
36 Gun Hill, Shale and Shale Grit.
37 Cloud End, coarse Grit Rock of the upper Coal series.
38 Biddulph Moor, Shale and Shale Grit.
39 Wickenstone Rocks, coarse Grit of the upper Coal series.
40 Golden Hill, upper Coal Measures.
41 Mole-copt Hill, coarse Grit Rock of the upper Coal series.

North Idle *Ridge*, from Wickersley Hill (No. 31), on East Rother Ridge, extends N.E. beyond the limits of the Map. (Blue.)

No.

1 Maltby Hill, yellow Lime.

No.

2 Sandbeck Park, yellow Lime; Roche-Abbey quarries near.

South Idle *Ridge*, from Sutton Hill (No. 54), on East Derwent Ridge, extends E. beyond the limits of the Map. (Vermilion.)

No.

1 Cocksmoor Clump, or Robin Hood Hills, Quartz Gravel, on yellow Lime, very high.
2 Sutton Forest Hill, Quartz Gravel, very high, a Station (Sutton Ashfield) in the Government Trigon. Survey.

No.

3 N. Hamilton Hill, Quartz Gravel on yellow Lime.
4 N. Mansfield Windmills-hill, Quartz Gravel and Gravel Rock, on yellow Lime.

East LEEN *Ridge*, extending from Hill No. 5, on South Idle Ridge, S. about 11 Miles. (Green.)

No.		No.	
1	Queens Bower, Quartz Gravel.	4 W.	Red Hill, Red Marl.
2	Langton Arbour, QuartzGrav.	5	Nottingham Windmill-hill, Quartz Gravel.
3	Holland Hill, Red Marl, high, a Station (Hollan) in the Government Trig. Survey.	6 E.	Castle Hill, Gravel Rock.

East MANIFOLD *Ridge*, extending from Axe-edge Hill (South, No. 32), on the Grand Ridge, S.S.E. about 15 Miles. (Green.)

No.		No.	
1	Summer Hill, Shale and Shale Grit.	12 E.	Steep Low, a hummock of 4th Lime.
2	White-shaw Hill, Shale and Shale Grit.	16	Ecton Hill, Shale Limestone, high, Copper Mine.
4	Edge Top, Shale and Shale Grit.	18 W.	Thor's House Tor, 4th Lime, a natural Arch and Cave.
5	Sheen Hill, a hummock of 1st Grit, on Shale.	19	Ilam Low, 4th Lime, high.
6	Bank Top, Shale & Shale Grit.	20	Bunster.Hill, 4th Lime, slither.

West MANIFOLD *Ridge*, extending from Brown Edge (No. 1,) on West Churnet & Dove Ridge, S.E. 7 Miles. (Green.)

No.		No.	
1	Riletch Hill, Shale Limestone, and Copper Mines.	3	Hurs Low, Shale Limestone.
2 E.	Revedge, a hummock of 1st Grit, on Shale.	5 W.	Old-field Low, 4th Lime.
		6 W.	Pike Low, 4th Lime.

North MEASE *Ridge*, extending from the South Mease Ridge, near Cole Orton (a branch from the West Soar Ridge), W. about 17 Miles. (Yellow.)

No.		No.	
1	Cole Orton Hill, Coal Shale.	18 S.	Union Hill, Grit.
2	Spring Wood.	19	Church Gresley Town, Coal Shale.
3 N.	Coppy Nook, red Clay.	20 N.	Cadley Hill, Quartz Gravel.
4 N.	Breedon Church Hill, yellow Lime.	21	Linton Village, Gravel, on red Marl.
5	Old Park.	22 S.	Over Seal Town, Leicest. Red Marl.
6	Smithsby Common, alluvial Clay on red Clay.	23 S.	Seal Grange, Red Marl.
7 N.	Pistern Hill, Grit.	24	Coton Park, Red Marl.
8 N.	Cadhouse Lane, high.	25 N.	Rosleston Park, Gravel on Red Marl.
9 N.	Derby Hills.	26 S.	Lullington Town, Gravel on Red Marl.
10 N.	King's Newton, Red Marl.	27 S.	Ladylee Hill, Red Marl.
11 N.	Bond-wood Hill, Gravel.	28	Burrow Fields, Red Marl.
12 N.	Askew Hill, Red Marl.	29 N.	Burrow Hill, Red Marl.
13	Butt House, red Clay.	30	Croxall Hill, Red Marl.
14 S.	Willesley Wood.		
15 S.	Donisthorpe.		
16	Wooden Box, red Clay.		
17 S.	Warren Hill, Grit.		

South

South MEASE *Ridge*, extending from near Cole Orton (a branch from the West Soar Ridge), W. about 20 Miles. (Green.)

No.
1 Alton Grange, Grit.
2 Normanton on the Heath, Red Marl.
3 Roe Hill, Red Marl.
4 S. Orton on the Hill.
5 Honey Hill, Red Marl.
6 S. Chilcote Hill, Red Marl.
7 ThorpeConstantine, Red Marl.
8 S. Clifton-Campville Church-Hill, Red Marl.
9 Gorse Hill, Red Marl.
10 Petvcroft Hill, Red Marl.
11 S. Elford Low, Red Marl.
12 S. Elford Hill, Red Marl.
13 Hazlehour Hill, Red Marl.

East MORLEDGE *Ridge*, extending from Hulland Ward Village (No. 83), on West Derwent Ridge, S. E. about 8 Miles. (Orange.)

No.
1 Cross-o'-th' Hands, Quartz Gravel, on Shale.
2 E. Flowry Hill, Quartz Gravel, on Shale.
3 Bullhurst, Quartz Gravel, on Shale.
4 Gun Hill, Quartz Gravel, on Shale.
5 Burley Hill, Quartz Gravel on Shale.
6 Quarn Hill, Quartz Gravel, on Shale.
7 Allestry Town Red Marl.
8 Darley Hill, Red Marl.

North NOE *Ridge*, extending from Kinder Scout Hill (No. 17), on the Grand Ridge, E.S.E. 8 Miles. (Green.)

No.
1 Seal-stone Hill, 1st Grit.
2 S. Grindlow Rime, a hummock of 1st Grit.
3 Crookston Hill, and Knowl, 1st Grit.
4 Win Hills, 3 hummocks of 1st Grit, on Shale and Shale Grit.

South NOE *Ridge*, extending from near Litton Edge (No. 24), on East Wye Ridge, N.E. 3½ Miles. (Lake.)

No.
1 Bur Tor, Shale and Shale Grit, a great Slip.
2 S. Sir William Hill, 1st Grit, very high.
3 S. Rock Hall, a hummock of 1st Grit.
4 S. Riley Hill, Shale & Shale Grit.
5 S. Cockey Low, Shale and Shale Grit.
6 Blakelow, Shale & Shale Grit.
7 Shatton Edge, Shale and Shale Grit.
8 S. High Low, Shale and Shale Grit.

East NUTBROOK *Ridge*, extending from Roby Field (No. 63), on East Derwent Ridge, S.E. about 6 Miles. (Lake.)

No.
1 Heanor Windmill-hill, Quartz Gravel, on Coal Shale.
2 Shipley-wood Hill.
3 Ilkeston, Church Hill, Grit.
4 Little Hallam Hill, Grit.

East ROTHER *Ridge*, extending from Over Moor (No. 52), on East Derwent Ridge, N. about 26 Miles. (Yellow.)

No.
1 Dunshill, Coal Shale.
2 Hardwick Park, yellow Lime.
3 Alt Hucknal Village, yellow Lime.

4 Glap-

No.		No.	
4	Glapwell Summerhouse, yellow Lime.	18 W.	Spinkhill Windmill-hill, 11th Grit.
5 E.	Cuckney Hay, Quartz Gravel, on yellow Lime.	19 W.	High Moor, Coal Shale.
6	Bolsover Town, yellow Lime.	20	Wales Town, Yorksh. Grit
7 E.	Bolsover Moor, yellow Lime, Quarries.	21	Kiveton Park.
8 E.	Deadmans-grave Wood, Quartz Gravel, on yellow Lime.	22 E.	South Anston Hill, yellow Lime and salmon coloured Grit, the Great Fault crosses it.
9 W.	Nuns Hill, Grit, perhaps the Wickersley Rock, Grindstone Quarry.	23	Todwick Grange, salmon-coloured Grit.
10	Barlborough Town, yel. Lime.	24 W.	Park Hill, 13th Grit.
11 W.	Marston Moor, 13th Grit.	25	Ulley, salmon-coloured Grit.
12	Knitaker Hill, yellow Lime, on Coal Measures.	26 W.	Bole Hill, salmon-coloured Grit.
13 E.	Holly Hill, yellow Lime.	27 E.	Laughton le Morthen Town, Yorksh. yellow Lime, high.
14 E.	Whitwell Hill, yellow Lime.	28 E.	North Anston Hill, yellow Lime.
15 E.	Whitwell Common, yellow Lime.	29 E.	Thurcoft Hill, yellow Lime.
16 E.	Winney Lane Hill, yellow Lime and salmon-coloured Grit, the Great Fault crosses it.	30	Morthen, Grit.
17 E.	Harthill Town, Yorkshire, salmon-coloured Grit.	31	Wickersley Hill, Grit, Grindstone Quarries.
		32	Boston Castle, salmon-coloured Grit.

West ROTHER *Ridge*, extending from Shepherd's Moss-House Hill (No. 27), on East Derwent Ridge, N.E. about 14 Miles. (Lake.)

No.		No.	
1	Lygate Hill, 4th Grit.	13 E.	Onestone Hill, a hummock of 11th Grit.
2	Holmsfield Village, 5th Grit.	14	Herding Hill, 7th Grit, high.
3 E.	Hill Top, 9th Grit, and 8th Coal Shale.	15 E	High Lane Hill, 9th Grit.
4 E.	Highfield Farm, 9th Grit, high.	16 E.	Berley Common, 11th Grit.
5	Bradway Cross, 6th Grit.	17 E.	Mosborough Hall, 11th Grit.
6 W.	Bole Hill, 6th Grit.	19	Woodthorp Hill, 11th Grit.
7 E.	Cole Aston Village, 9th Grit.	20 W.	Sheffield Manour, (Summer-house), 11th Grit.
8 E.	Moor Top House, 10th Grit.	21 E.	Hansworth Town, Yorkshire, 12th Grit.
9 E.	Bramley Moor, 9th Grit.	22 E.	Howth Hill, salmon coloured Grit.
10 E.	Renishaw Hill, 10th Grit.		
11 E.	Middle Handley, 9th Grit.		
12 E.	Glasshouse Common, 9th Grit.		

North SCHOO *Ridge*, extending from Harboro Rocks (No. 74), on West Derwent Ridge, S.W. about 8 Miles. (Lake.)

No.		No.	
1	Larle Tor, 4th Lime.	4 N.	Heaver Clump, Shale Limestone.
2	Kniveton Hill, Shale Limestone.	5	Ashburne Windmill-hill, Shale.
3 S.	Maghill-Bowse Clump, Shale Limestone, high.		

South SCHOO *Ridge*, extending from Gib Hill (No. 81), on West Derwent Ridge, W.S.W. about 8 Miles. (Yellow.)

No.		No.	
1 S.	Hough Park Clump, Quartz Gravel, on Shale.	2	Bradley Park Clump, Quartz Gravel, on Shale.

3 S. Yelders-

No.
3 S. Yeldersley Wood, loamy Quartz Gravel, on Coal Measures?
4 S. Shirley Common, Quartz Gravel, on Coal Measures?
5 S. Hollington Village, red Marl.
6 Edlaston Town, loamy Quartz Gravel, on Coal Measures?
7 S. Alkmanton Village Red Marl.
8 S. Boylstone Hill Red Marl.
9 S. Hoon Hill (and Mount), Red Marl.

No.
10 Snelston Common, loamy Quartz Gravel, on Coal Measures?
11 S. Birchwood Park, yellow Lime?
12 S. Birchwood Moor, sandy Gravel, on Coal Measures?
13 S. Hare Hill, and Clump, Quartz Gravel, on Red Marl.
14 S. Marston Park, Red Marl.
15 S. Upwood, Red Marl.

North SHELF *Ridge*, extending from Shelf Stones (No. 11), on the Grand Ridge, W. about 4 Miles. (Yellow.)

No.
1 Glossop Low, 1st Grit, Gray Slate Quarries.

No.
2 Mouse Low, 2d Grit.

South SHELF *Ridge*, extending from Glead Hill (No 14), on the Grand Ridge, nearly W. about 5 Miles. (Blue.)

No.
1 Hollingsworth Head, 1st Grit and 1st Coal Shale.
3 Combs Rock, 3d Grit.
4 S. Picking Stones, 3d Grit.
5 S. Ernocroft Barrow, 3d Grit.

No.
6 S. Cobden Edge, 4th Grit, Quarries, high.
7 S. Eaves Knoll, a hummock of 4th Grit.

East SOAR and EYE *Ridge*, extending from the Grand Ridge, near Walton in Leicestershire, to Red Hill at the junction of the Soar and Trent Rivers. (Blue.)

No.
1 Red Hill, Red Marl, and Gypsum Quarries.
2 Fox Hill, Red Marl.

No.
3 Hotchley Hill, Clay on Lias Limestone.

West SOAR *Ridge*, extending from the Grand Ridge near High-Cross in Warwickshire, nearly N. about 33 Miles. (Vermilion.)

No.
1 Barlstone Windmill hill, Red Marl.
2 E. Desford Hill, Red Marl.
3 E. Birstal Cliff-house, alluvial Clay, Flints, &c. on Red Marl.
4 E. Wanlip Hill, Red Marl.
5 Green Hill, coarse Slate.
6 E. Cliff Hill, Sienite.
7 Bardon Hill, coarse Slate and Sienite, very high, a Station in the Government Trigonometrical Survey.
8 E. Osgathorpe Hill, Red Marl

No.
9 E. Belton Town, Leicestershire, red Marl.
10 E. Hathern Hill, red Marl.
11 Swannington Hill, Coal Shale.
12 E. Barrow Hill, yellow Lime.
13 W. Clouds Hill, yellow Lime.
14 Coppy Hill, red Marl.
15 W. Donnington Park, red Marl.
16 Donnington Windmill-hill, red Marl.
17 E. Whatton Hill, red Marl.
18 E. Kegworth Windmill-hill, red Marl.
19 Castle Hill, red Marl.

East

East WYE *Ridge*, extending from Rushop Edge (No. 21), on the Grand Ridge, S.E. about 17 Miles. (Yellow.)

No.
1 Lords Seat, Shale and Shale Grit, very high, a Station in the Government Trigonometrical Survey.
2 Mam Tor, Shale great slips.
3 Lose Hill, Shale and Shale Grit; the N.W. end called Back Tor, is much slipt.
4 Windy Knowl. 4th Lime.
5 E. Tracliff Hill, 4th Lime, Fluor Spar Mines in it.
6 E. Castle Hill. 4th Lime.
7 W. Elden (Little) Hill, 4th Lime.
8 W. Elden (Great) Hill, 4th Lime, Elden Hole.
9 E. Hentley Hill, 3d Lime.
10 E. Edingtree Hill, 3d Lime.
11 E. Eccles Hill, Shale and Shale Grit.
12 Long Cliff, 4th Lime.
13 W. Oxlow, 4th Lime.
14 Copt Round, 3d Lime, very high.
15 W. Nels Low, a hummock of 3d Lime.
16 W. Damcliff, 4th Lime.
17 W. Whetstone Bank, 3d Lime.

No.
18 W. Summercross Hill, 2d Lime, high.
20 W. Knot Low, 3d Lime.
21 W. Tenter Bank, a hummock of 2d Lime.
22 Batham Edge, 3d Lime.
23 Tidslow Top, 3d Lime, high.
24 W. Litton Edge, 1st Lime.
25 Longrood Hill, 1st Lime.
27 W. Wardlow Top, 1st Lime.
28 W. Wardlow Hay. 1st Lime.
29 W. Scrat Tor, 1st Lime.
30 Blakelow Tor, 1st Lime, very high.
31 E. Beacon Rod, 1st Lime.
32 E. Calver Peak, 1st Lime, great Quarries.
33 W. Fin Copt, 1st Lime, similar strata to Matlock High Tor, Section, Plate II. vol. xxxi.
34 Crackendale Head, 1st Lime.
35 Bowcross, Shale and Shale Grit, Freestone Quarries.
36 Cawton Hill, Shale and Shale Grit.
37 W. Haddon Park, Shale and Shale Grit.

South WYE *Ridge*, extending from Hill No. 21, on West Derwent Ridge, E. about 12 Miles. (Lake.)

No.
1 Blindow Top, 3d Lime.
2 Chelmerton Thorn, 3d Lime.
3 Sough Top, 2d Lime, high.
4 N. Chelmerton Low, 2d Lime, very high.
5 N. Cawton Hill, alluvial Toadstone? on 3d Lime.
6 N. Chee Tor Hill, 4th Lime, tilted.
7 Wam Rake Head, 2d Lime.
8 Slipper Low, 2d Lime.
10 N. Priestcliff Lees, 2d Lime.
11 N. Priestcliff Low, 2d Lime, and 2d Toadstone.
12 N. Pet Hill, 2d Lime, and 2d Toadstone.
13 N. Wood Head, 2d Lime.

No.
14 Dimins-dale Top, 2d Lime.
15 High Low, 1st Lime, Bird's eye, Marble Quarry.
16 Sheldon Town head, 1st Lime.
17 Shack Low, 1st Lime.
18 Bole Hill, 1st Lime, an old Lead Hearth.
19 S. Ricklow-dale Head, 1st Lime,
20 N. Dirtlow Hill, Shale Limestone, Rottenstone pits.
21 N. Harrack Hill, Shale Limestone.
22 Nether Haddon Pasture-head, 1st Lime.
23 Haddon Field, 1st Lime and Shale, a Limestone Quarry.

On comparing the above, with the alphabetical printed list in the Report, I am sorry to observe, that errors in the numbers, &c. have unfortunately escaped correction, in Aldwark, Alise, Barn, Brislincote, Broadhurst, Brownmiddle, Coneygree, Dunrake, Heath-Hill, Old-Booth, Seven, Sheepston, and Underwood, which I have noticed in parentheses

rentheses in the above list, for future correction. It may be proper to add, that the nine lowest of the *Strata* mentioned in this list, are shown in order, with their average thicknesses, in the section in plate II. in your thirty-first vol. The shale limestone and shale grit, are accidental or partial strata, or form chance, or anomalous beds in the limestone shale, there shown: the grit rocks and coal-shales, follow each other in alternate succession *upwards*, from the 1st grit, the uppermost shown in this section; always remembering, that a coal-shale of the same number is *above* every grit rock, and a toadstone of the same number *under* every lime rock, except perhaps, the 4th or lowest, whose under strata are wholly unknown in Britain, I believe. The salmon-coloured grit of Rotherham, South-Anston, and an intermediate line of places in Yorkshire, is perhaps, the 16th grit, and the Wickersley grindstone rock is above this, in the succession of numbered rocks and coal shales, which all terminate, under the yellow lime strata, whose edges occupy the surface from near Nottingham to Wetherby, and further in Yorkshire, without the regular upper strata to them anywhere appearing, owing to the gravel, in any place which I have visited. A great succession of strata, including two or three important series of coal-measures, are supposed to occur between the yellow lime and the red marl or red earth in ascending the series. The red marl contains accidental or partial strata, &c. of grit-stone, gypsum, limestone, sienite, slate, rock salt, &c. as I hinted at page 40, of your thirty-first volume above quoted, but where, by a press error, they are called "concoctions," instead of concretions or nodules. The Balderton sand, and part of the Lias strata*, form the highest strata which are anywhere noticed in this List of Hills, excepting only the gravels, which are superficial, and accidentally distributed on all the strata of the district.

XXXI. *De-*

* See vol. xxxvi. p. 105. And while referring to this part of your Magazine for August last, permit me to mention, that in consequence of my inquiries therein, after the geological Papers of the late Rev. John Michell, Sir Thomas Turton, Bart, the son-in-law and executor of Mr. Michell's will, very politely wrote to inform me, that a search among his papers had discovered none relating to the strata of England, as I had hoped, and expected: my hopes, and that of other geological inquirers, are therefore, now the more fixed, on the gentlemen who have the *Woodwardian* Collection and Papers, in care, to inform us, of what they may contain, as to the *ascertained succession of strata in England*; a search which, surely is justified, by the following passage, addressed by Mr. B. Holloway to Dr. Woodward, when writing him an account of the pits of Fullers' Earth in Bedfordshire, after mentioning several places in distant counties where the chalk and the Woburn Sand strata range, viz.

"This I take notice of, because it confirms *what you say*, of the *regular disposition*

XXXI. *Description of a Burning Mirror, by means of which we may reflect and fix on any Object, whether at Rest or in Motion, the solar Rays in as great a Quantity as we please. By* F. Peyrard, *Professor of Mathematics in the Bonaparte Lyceum. Translated from the French.*

[Concluded from p. 146.]

My mirror is exempt from all these defects; for, in proportion as the sun advances, the glasses do not cease to form a parabolic mirror, the axis of which is constantly directed to the sun's centre, passing by the object which we wish to inflame: *i. e.* at each instant my mirror changes its form in order to produce its effect.

Before Buffon, Athanasius Kircher contrived a mirror for burning at 100 feet and upwards. His mirror was a collection of plain and circular glasses; he placed them on a wall, giving them a proper inclination, in order that the images of the sun might be reflected on the same object.

Athanasius Kircher made his experiments with five glasses only: he informs us that the heat produced with four glasses was still supportable, and that the heat produced with five was almost insupportable. "I am of opinion," he adds, "that it was with plain mirrors thus arranged that Proclus burned the ships of Vitalian."

Kircher did not push his experiments further, and contented himself with inviting the learned to repeat them with a greater number of glasses *.

It is almost unnecessary to observe that the above has all the defects of Buffon's mirror.

Anthemius of Tralles, who flourished about the end of the fifth century, and was intrusted by Justinian the First, with the building of the temple of St. Sophia at Constantinople, also contrived a burning mirror. A fragment of his, in

position of the earth into like strata or layers of matter, commonly *through vast tracts*."—Phil. Trans. No. 379, or Reid and Gray's Abridgement, vol. vi. p. 133.

In consequence also of my inquiries, by the same channel, after the papers and maps of the late Mr. Tofield, relating to the British strata, William Smithson, Esq. of Heath near Wakefield, procured a letter to be written to Mr. William Tofield of Wath-upon-Derne, a younger son of Mr. T. and has transmitted me his answer, which unfortunately states, that all his father's manuscripts were by his elder brother taken with him into Holland, where he has resided for 18 years past, and the state of the war has, for some years, prevented any communication with him.—J. F.

* Kircher *De arte magna lucis et umbræ*, lib. x. par. 11 probl. 4.

in which he gives a description of it, was translated by M. Dupuy, and is to be found in the Memoirs of the Academy of Inscriptions and Belles-Lettres for 1777. The following are his own words:

"*Construct a Machine capable of burning at a given Distance by means of Solar Rays.*

"This problem appears impossible, if we adhere to the idea of those who have explained the method of constructing what are called burning mirrors: for we always find that these mirrors look towards the sun when inflammation is produced; so that if the given spot be not on the same line with the solar rays, if it incline one way or the other, or if it be in an opposite direction, it is impossible to execute what we propose by means of burning mirrors. Besides, the size of the mirror, which ought to be proportioned to the distance to which it is required to send the fire, forces us to acknowledge that the construction, as explained by the ancients, is almost impracticable. Nevertheless, as we cannot take away from Archimedes the glory which is due to him, since it is unanimously allowed that he burnt the enemy's ships by means of the solar rays, reason compels us to admit that the problem is possible. For my part, after having examined the subject, after having considered it with all the attention of which I am capable, I shall explain the method which theory has suggested, after offering a few preliminaries.

"*At a given point of a plain Mirror, find a position, such as a Solar Ray coming from any inclination whatever strike this point, and let it be reflected at another point which is also given.*

"Let A (fig. 5) be the point given, BA the ray given, according to any direction; and let it so happen that the ray BA, falling on a plain mirror and attached to the point A, is reflected at the point given Γ.

"Draw from the point A to the point Γ the straight line AΓ: divide into two equal parts the angle BAΓ by the straight line AΔ, and conceive the plain mirror EAZ in a situation perpendicular to the line AΔ, it is evident, from what has been demonstrated, that the ray BA falling on the mirror EAZ will be reflected at the point Γ; all which must be executed ......

"Consequently all the solar rays equally inclined, and falling parallel to AB on the mirror, will be reflected by parallel lines at AΓ. It is therefore demonstrated that, on

 what-

whatever side the point Γ is, in whatever position it is with respect to the solar ray, this ray will be reflected on the same side by the plain mirror." But inflammation does not take place by means of burning mirrors, unless when several rays are collected in one and the same place, and when the heat is condensed to the burning point. It is thus that, when a fire is kindled in any place, the parts adjacent and the circumambient air receive a proportionate degree of heat. If, therefore, we conceive that on the contrary all these degrees of heat are collected and concentrated at this place, they will there exert the force of the fire just mentioned. Let us, therefore, bring and collect at the point Γ removed from the point A the distance which we have assigned to it, other different rays, by means of plain and similar mirrors, in such a manner that all these rays, united after reflection, may produce inflammation: this may be effected by means of several persons holding mirrors, which, according to the position indicated, send the rays to the point Γ......

"But in order to avoid the embarrassment resulting from intrusting this operation to several persons, for we shall find that the matter intended to be burnt does not require less than 24 reflections, the following construction must be followed:

"Let ΑΒΓΔΕΖ be the hexagonal plain mirror, and other adjoining similar mirrors, and attached to the first according to the straight lines ΑΒ, ΒΓ, ΓΔ, ΔΕ, ΕΖ, &c. (fig. 6), by the smallest diameter, so that they may be moved on these lines, by means of plates or bands applied, which unite them to each other, or by means of what are called hinges. If, therefore, we bring the surrounding mirrors into the same plane with the mirror in the centre, it is clear that all the rays will undergo a reflection similar and conform to the common position of all the parts of the instrument. But if, the centre mirror remaining as it were immoveable, we dexterously incline upon it all the other mirrors which surround it, it is evident that the rays reflected by them will tend towards the middle of the place where the first mirror is directed. Repeat the same operation, and around the mirrors already described, placing other similar mirrors, all of which may be inclined towards the central mirror, collect towards the same point the rays which they send, so that all these united rays may excite inflammation in the given spot.

"But this inflammation will take place better if you can employ for this purpose four or five of these burning mirrors,

rors, and even seven, and if they are all at the same distance from the substance to be burnt, so as that the rays which issue from them, mutually intersecting, may render the inflammation more considerable. For if the mirrors are all in one place, the rays reflected will intersect at very acute angles; so that all the place around the axis being heated, the inflammation will not take place at the single point given. We may also, by means of the construction of these same plain mirrors, dazzle the eyes of the enemy's forces, who, not perceiving those who carry them on their shields, or above their heads, will fall into confusion.

"It is therefore possible, by means of the burning mirrors just mentioned, to carry inflammation to a given distance. Those who have made mention of the mirrors constructed by the divine Archimedes have not said that he made use of a single burning mirror, but of several; and I am of opinion that there is no other way of carrying inflammation to any distance.

"But as the ancients, in treating of common burning mirrors, have not explained in what manner the embola must be traced except by an organic process, without presenting on this head any geometrical demonstration, without even saying that they were conic sections, nor of what kind, nor how they were formed, we shall attempt to give some descriptions of similar embola, not without demonstration, but by geometrical proeesses.

"Let AB therefore (fig. 7) be the diameter of the burning mirror which we wish to construct, or upon which we wish to operate, and upon the line ΓΕΔ, whieh cuts perpendicularly the line AB into two equal parts, let Δ be the point where we wish the reflection to be made; the point E being the middle of the line AB. Join B, Δ, and by B let there be drawn to ΔΕΓ the parallel BZ equal to BΔ; by the point Z, the line ZΓ parallel to BA, cutting at the point Γ the line ΔΕΓ. Cut by the middle ΓΔ to the point Θ, and ΘE will be the height of the embola relative to the diameter AB, as we shall presently see. Divide into as many equal parts as you please the straight line BE; into three, for instance, as in the figure subjoined: viz. EK, KΛ, and ΛB, and by the points K, Λ, draw at BZ, EΓ, the parallels ΛM, KN. Afterwards divide into two equal parts the angle ZBΔ, by the straight line BΞ, the point Ξ being considered as in the middle between the parallels BZ, ΛM. Prolong all these parallels from the side of Δ towards the points Π, P, Σ, I say that the ray parallel to the axis, *i. e.* to EΔ, and falling by ΣB on the mirror at the point B, will be

be reflected at the point Δ, on account of the angle ZBΔ being divided into two equal parts, and the reflection being made at equal angles, as we have already shown.

"We shall reflect in a similar manner in Δ the ray PA in this manner: Draw the straight lines ΞΔ, ΞM, ΞZ. It is evident that ΞΔ is equal to ΞZ, on account of the angle at B being divided equally into two parts. But ΞZ is equal to ΞM, because from the middle point Ξ they are directed towards the points Z, M. Thus MΞ is equal to ΞΔ. Cut, therefore, into two equal parts the angle MΞΔ by the line ΞTO, the point O being considered in the middle between the parallels MA, NK; and this line secting the parallel MA at the point T; we shall demonstrate by the same reasons that MT is equal to TΔ, and that TΔ .... *Cætera desunt.*"

The mirror of Anthemius, like that of Buffon, has all the properties, and nothing but the properties, of a parabolic mirror composed of plain glasses. Both these mirrors may set fire to an object, whatever its position may be. The mirror of Anthemius, which is geometrically constructed, is a true parabolic mirror; whereas the mirror of Buffon, when it is adjusted, is a very imperfect parabolic mirror. The focus of the parabolic mirror of Anthemius is invariable; whereas the focus of Buffon's mirror is variable at pleasure. But we should be strangely deceived if we thought that, the position of the object being given and the position of the mirror being also given, we could set fire to an object at any hour of the day or any day of the year. These two mirrors cannot produce all their effects, except at the very moment when the sun is at the same point in the heavens at which it was when the mirror of Anthemius was constructed, and when that of Buffon was adjusted.

It now remains to speak of the burning mirror of Archimedes, with which he is said to have burned the fleet of Marcellus before the walls of Syracuse.

The ancient authors who speak of this mirror are Lucian, Galienus, Anthemius of Tralles, Eustathius, Tzetzes and Zonaras.

Lucian says, in his Hippias, that Archimedes, by a singular artifice, reduced the ships of the Romans to ashes.

Galienus expresses himself in the following manner: "It is in this way, at least I think so, that Archimedes burnt the enemy's vessels. For, by the help of a burning mirror, we may easily set fire to wool, hemp, wood, &c. and, in short, to any thing dry and light *."

* *De Temperamentis*, lib. iii. cap. 2.

Anthemius,

Anthemius, who flourished at the commencement of the sixth century, informs us that it was unanimously allowed that Archimedes burned the enemy's vessels by means of the solar rays.

Eustathius, in his commentary on the Iliad, says that Archimedes, by an invention in catoptrics, burned the fleet of the Romans at a distance equal to the shot of an arrow from a bow.

"Archimedes," says Zonaras (*Annal.* lib. ix.) "burnt the fleet of the Romans in an admirable manner; for he turned a certain mirror towards the sun. and which received its rays. The air having been heated on account of the density and smoothness of this mirror, he kindled an immense flame, which he precipitated on the vessels which were in the harbour and reduced them to ashes."

"When the fleet of Marcellus was within bow-shot," says Tzetzes, "the old man (Archimedes) brought out a hexagonal mirror which he had made. He placed at proper distances from this mirror other smaller mirrors which were of the same kind, and which were moved by means of their hinges and certain square plates of metal. He afterwards placed his mirror in the midst of the solar rays, precisely at noon day. The rays of the sun being reflected by this mirror, he kindled a dreadful fire in the ships, which were reduced to ashes at a distance equal to that of a bow-shot. . . . .Dion and Diodorus, who wrote the life of Archimedes, and several other authors, speak of this fact; but chiefly Anthemius, who wrote on the prodigies of mechanics; Hero, Philo, Pappus, and in short all who have written on ancient mechanics: it is in these works that we read the history of the conflagration occasioned by the mirror of Archimedes."

Such are the authorities on which the history of the burning mirrors of Archimedes is founded, and these authorities are in my opinion of great weight. The silence of Polybius, of Livy, and of Plutarch, however, who relate at great length what Archimedes did for the defence of Syracuse, seems to warrant a doubt as to the fact of the burning the fleet of Marcellus. But whether Archimedes did or did not burn the fleet of Marcellus, it is certain that he contrived a burning mirror, and that this mirror was an assemblage of plain mirrors.

But what was the burning mirror of Archimedes? I shall endeavour to answer this question, after I shall have made some observations on the different kinds of parabolic mirrors composed of plain glasses.

 Take

Take a parabolic conoid, the axis of which is constantly directed to the centre of the sun: let us suppose that some plain glasses are tangent to this conoid, and suppose that this conoid is cut by a vertical plane which passes by its axis. If we cut this conoid by a plane perpendicular on the axis, we shall have, on the side of the summit, a burning mirror composed of plain glasses which will not inflame an object, except in so far as it is placed directly between the mirror and the sun. If we cut the conoid by a plane which is perpendicular on the vertical plane, and which passes between the sun and the zenith, the upper segment will give a burning mirror which will set fire to an object from top to bottom; and the other segment will give a mirror which will set fire to it from bottom to top, provided this object is in the vertical plane of which we have been speaking. Lastly, let us suppose that the secting plane is not perpendicular on the axis, and that it forms with the horizon an acute angle, whether the secting plane passes by the axis, or whether it cuts or does not cut the axis, one of the burning mirrors which will result from this section will inflame from top to bottom, the other from bottom to top, any object which is placed to the right or left of the sun, and this is the case with the mirror of Anthemius as well as with that of Buffon.

This being granted, let us return to the burning mirror of Archimedes. Anthemius relates that in the descriptions which the ancient authors gave of burning mirrors, we always find that these mirrors faced the sun, when the inflammation was produced, and that the object set fire to was neither to the right nor to the left. Hence I conclude, that the mirror of Archimedes was one of the segments of the parabolic conoid just mentioned, when the secting plane is perpendicular on the vertical plane.

Tzetzes informs us that the mirror of Archimedes was a collection of hexagonal mirrors, which moved by means of their hinges and certain plates of metal, *i. e.* that the mirrors of Archimedes were arranged in such a way that each could be moved in every direction, as in Buffon's mirror; and so far the mirror of Buffon does not differ from that of Archimedes, except in so far as the mirrors being rectangular in the former and hexagonal in the latter.

Tzetzes adds that Archimedes placed his mirror in the midst of the solar rays at noon day: *i. e.* he placed his mirror perpendicular to the plane of the equator. If the mirror of Archimedes had not been intended to produce inflam-

inflammation, except at the moment when the sun was in a plane perpendicular upon the plane of the mirror and the plane of the horizon, it is evident that it would have been indifferent whether this mirror was or was not placed perpendicularly on the plane of the equator. But wherefore did Archimedes place his mirror perpendicularly on the plane of the equator? It was in order that his mirror might reflect the solar rays on the same object during the whole time that the sun was on the horizon, and I shall show that the mirror thus placed was capable of producing this effect in two different ways.

Let AB (fig. 8) be an iron rod parallel to the axis of the globe. Let CD be an iron rod perpendicular upon AB; let EF be the mirror of Archimedes, and let it be placed in such a way that the iron branch CD is perpendicular on its plane when prolonged. It is evident that this mirror, when so placed, will be perpendicular on the plane of the equator. Let us suppose that by means of a screw, as we see in fig. 9, we can move the iron rod AB on itself. This being done, let any person, on turning the screw, be desired to keep the mirror in a position perpendicular on the vertical plane which passes by the axis of the iron rod AB and by the centre of the sun, and that another person is employed to adjust the mirror in such a way that the images reflected may be carried to a point D, taken on the iron rod CD.

If during the whole day we retain by means of the screw the mirror in a position perpendicular on the vertical plane which passes by the axis of the iron rod AB and by the centre of the sun, it is evident that the images reflected at the point D will remain there fixed, without vibrating and without displacing the focus: for, if the contrary happened, it would be because in the space of twelve or fifteen hours the sun would approach or remove from the equator in a sensible manner; which is not the case.

In the second place, let there be a piece of iron ACDEB (fig. 9): let its extremities AC, EB, be cylindrical, and let the part CDE be flattened and bent into a semicircle; let the axes of the cylinders AC, EB, be in the straight line AB, and let this straight line be parallel to the axis of the earth; let the piece of iron ACDEB be moveable around the axis AB, and let LI be a screw; let DK be the mirror of Archimedes; let this mirror be placed parallel to AB and perpendicular to the plane which passes by the axis of

the straight line AB and by the point D, the middle of the band CDE. It is evident that the mirror DK will be placed perpendicularly to the plane of the equator.

This being done, let a person by turning the screw KL be instructed to keep the mirror in a position perpendicular on the vertical plane which passes by AB, and by the centre of the sun, and let another person adjust the mirror so as that the images reflected may be borne on a point L of the axis. The mirror being adjusted, it is evident that the images reflected at the point D will there remain fixed during the whole of the time that the sun is upon the horizon.

By means of a quadrant GG and a needle fixed with the axis AB, it will be easy, the hour of the day being known, to keep the mirror in its proper position.

I have demonstrated that, the burning mirror of Archimedes remaining perpendicular on the plane of the equator, it would be possible to fix on an object the solar images during the whole of the time that the sun was upon the horizon, and I have shown that this might be done in two ways. But it is evident that with the constructions which I have given, the thing is not physically possible, except when the distance between the object to be inflamed and the mirror does not exceed certain limits. It remains to show, that by modifying the second construction we may set fire to an object placed at a great distance.

While the straight line DK turns round the axis AB, the perpendicular drawn from the point K on AB engenders a circle parallel to the equator, and the straight line drawn from the point K parallel to AB engenders an ellipsis in the horizontal plane. Hence it follows that if we move the mirror DK in such a manner that this straight line prolonged moves according to the horizontal ellipsis, and that the point D is moved according to the circumference of the circle parallel to the equator, the plane of the mirror remaining always parallel to the axis of the earth and perpendicular upon the vertical plane which passes by the centre of the sun and by the centre of the mirror, it is evident that the images reflected by the mirrors would remain fixed at the point L, as before.

This being done, let us see how we should proceed to burn an object placed at a great distance.

The height of the pole and the distance of the object to be set fire to being known, the ellipsis which it is requisite

to

to trace upon the horizontal plane is determined. This ellipsis being traced, we move the mirror in the same way as in fig. 9, by means of a machine which it would be easy to contrive. Hence I conclude, that by following the same principles as before we may burn an object placed at a great distance. In this manner therefore Archimedes may have burnt the fleet of Marcellus.

It will be easy to perceive that the mirror EF (fig. 8) and DK (fig. 9) might have an oblique position on the plane of the equator, provided in the two cases it was fixed with the straight line AB perpendicular on the plane of the equator.

I shall conclude this essay with two observations. If the mirror DK, instead of having a fixed position, was moveable in the iron band CDE (fig. 9), and if this mirror was adjusted in order to carry the images to R the middle of CE, it is evident that if we made this mirror have its axis YZ constantly directed to the sun's centre, the focus would remain at the point R during the whole day and every day of the year.

I call the axis of a burning mirror the axis of the conoid, one part of the surface of which forms the burning mirror.

Upon the same principles it would be easy to make a mirror of refraction, so as to keep its focus constantly at the same point.

Let AB (fig. 10) be an iron rod parallel to the axis of the earth; let CDE be an iron band folded into the arc of a circle, having for its centre the point M taken on the axis of the rod AB; let KL be a glass moveable round an axis perpendicular on the plane which passes by AB and by the middle of the band CDE. Let us suppose that by means of a screw, we keep, during the whole time that the sun is upon the horizon, the glass parallel to the sun; it is evident that the focus Q will remain fixed at the same point of a hollow RDS placed on the band CDE.

XXXII. *On Tuning Musical Instruments.* By a CORRESPONDENT.

To Mr. Tilloch.

SIR, WITHOUT entering at all into the disputes about words, the characters of authors; what silly tuners do, or say; and whether *fifths* should be this or that way tempered,

pered, without adverting to the *thirds* and the *sixths* (where all the difficulties lie) on which Mr. Smyth and Mr. Merrick seem at issue in your Magazine; I beg the favour of you to give a place to the small Table of three columns in the margin; the 1st of which shows the numerical differences of lengths of strings, between Mr. Merrick's equal temperament scale by experiments on the melody and his calculated lengths, at page 113 of your last Number: the 2d, the notes: and the 3d, the differences in schismas and decimals between these notes respectively; as the same point out, I think, clearly, the source of Mr. M.'s mistaken assertions as to the truth of his tuning by this method; viz. the wrong estimate he forms of the power of such a monochord to decide, or rather, of the differences which the lengths of strings to 3 places or ·1000dths of the whole string, show: Would he otherwise have boasted of his accuracy in tuning an equal temperament fifth CG, which is very near 4 schismas *sharp*, instead of one schisma *flat*, as it ought to be, very nearly, as shown in the 6th scholium to Mr. Farey's useful theorem in your xxxvith vol. p. 47, or five times the real equal temperament, different from what it ought to be!! The fourth F ought on a 3 place monochord to be ·749, and not ·750; this note therefore agrees as well as B; but all the other 9 notes differ, in no instance less than 1, and in one more than 5 times, the interval which is the true or proper equal temperament of the *fifth*, by which the tuning is usually conducted, and on which so much stress is laid in the quotation from Dr. Chladni. From this Table, by observing what very different intervals a difference of 1 (or ·001) on this monochord gives in different parts of the octave, the folly of Lord Stanhope and others, in pretending that these differences of length of strings are proper measures of the intervals, will strikingly appear; and it can scarcely be necessary to add, that unless a monochord will correctly show 4 places of decimals of the whole string, it is useless and highly mischievous as a tuning apparatus; since, in some parts of the scale, *three* figures are incapable of showing the temperaments, within more than half of the whole temperament of each fifth and fourth, in the equal temperament scales. Surely, Mr. A. Coblenz and Dr. Bemetzrieder, or their like fellows, must have been the "experienced tuners," who assisted in this

		Σ
5	C*	4·7059*
5	D	4·9866*
5	D*	5·2850*
1	E	1·1167*
1	F	1·1828*
3	F*	3·7681*
3	G	3·9946*
2	G*	2·8176*
1	A	1·4880b
2	A*	3·1647*

this notable experiment, or they would have prevented Mr. M. from thus committing himself, and would have whispered him that most *practical* tuners, if they do not actually *count* the beats while tuning, make use of their recollection from habit, of the frequency which they have in each concord to be tuned, for adjusting the same, according to the temperament they have been taught, or are used to, and never resort to the *melody* in tuning. I beg to subscribe myself

London, 8th March, 1811. NO FRIEND TO TUNING QUACKS.

XXXIII. *A Method for ascertaining Latitude and Time by means of Two known Stars.* By M. DELAMBRE. *Translated from the* "Connoissance des Tems" *by Mr.* FIRMINGER, *many Years Assistant Astronomer at the Royal Observatory, Greenwich.*

[Concluded from p. 35.]

THE formula (5) is reduced to the term $-\frac{dh \sin A}{\cos \varphi} = d\lambda$, so that the case which appeared most unfavourable is on the contrary the easiest, and in which the error may be a minimum.

Let us suppose, lastly, that $A = A' = 90°$, or that the two stars have been observed at the first vertical: in this case the error will be nought.—In fact, $\cot \varphi = \cot \delta \cos \lambda = \cot \delta' \cos (\lambda - \varphi) = \cot \delta' \cos \theta \cos \lambda + \cot \delta' \sin \theta \sin \lambda$; or $\operatorname{tang} \delta' \cot \delta \cos \lambda = \cos \theta \cos \lambda + \sin \theta \sin \lambda \operatorname{tang} \delta' \cot \delta$ $= \cos \theta + \operatorname{tang} \lambda \sin \theta$, $\operatorname{tang} \lambda = \frac{\operatorname{tang} \delta' \cot \delta}{\sin \theta} - \cot \theta$, and $\cot \varphi = \cot \delta \cos \lambda$.

It is evident that the errors dh and dh' are nought, since h and h' need not be observed.

This case is, therefore, the truest of all, and the one that would be found most favourable, if it was easy to observe in the vertical. An *instrument des passages* (transit instrument) would be required for that purpose, directed east and west.

Let us see, lastly, the case wherein A', though not equal to it, differs from it a slight quantity: in this case x being equal to $A - A'$, x will be very small.

The formula (4) becomes $\frac{-dh \sin (A - x) + dh' \sin A}{\sin x} =$

$$\frac{-dh \sin A \times \cos x + dh \cos A \sin x + dh' \sin A}{\sin x} =$$

$$\frac{dh' \sin A - dh \sin A + 2dh \sin A \sin^2 \frac{1}{2} x + dh \cos A \sin x}{\sin x} = \frac{(dh' - dh) \sin A}{\sin x}$$

$+ dh \sin A \operatorname{tang} \frac{1}{2} x + dh \cos A$. $dh \cos$

dh cos A is the same as was obtained by supposing $x=o$; the term dh sin A tang $\frac{1}{2}x$ will be a very small trifle. As to the term $\frac{(dh'-dh)\sin A}{\sin x}$, it will be reduced to nought, or to very little. To nought, if $dh'=dh$; and the two altitudes being measured by the same instrument, we shall have $dh'-dh=o$, when the errors will be those of the line of collimation, which will happen in most cases.

It will be reduced at least to very little. Suppose $dh'-dh=10''$; $x=1^\circ$; $\frac{(dh'-dh)}{\sin 1^\circ}\sin A = 57''\sin A = 28''$, suppose A $=30^\circ$ by a mean between the extreme values of sine A.

If h alone has been observed, because $A=A'$, we shall then have $h'=h+D$; thus h' is supposed to be equal to $h+D$ and dh' to dh, therefore $dh'-dh=o$; in which case likewise $\frac{1}{2}$ tang $x=o$, there only remains dh cos A, which will never exceed dh.

The errors $d\lambda$ become in the same manner:

$$\frac{-dh\cos(A-x)+dh'\cos A}{\cos\phi\sin x}=\frac{-dh\cos A\cos x-dh\sin A\sin x+dh'\cos A}{\sin x\cos\phi}$$

$$=\frac{(dh'-dh)\cos A+2dh\cos A\sin^2\frac{1}{2}x}{\sin x\cos\phi}-\frac{dh\sin A}{\cos\phi}=\frac{(dh'-dh)}{\sin x}\frac{\cos A}{\cos\phi}$$

$$+\frac{dh\cos A\,\text{tang}\frac{1}{2}x}{\cos\phi}-\frac{dh\sin A}{\cos\phi}.$$

At first glance, the error on ϕ and λ should increase in proportion as the sine x lessens, and should be like infinite when $x=o$; still if $x=o$, the errors are reduced to dh cos A and $\frac{dh\sin A}{\cos\phi}$.

It might, therefore, be thought that such is the error when arrived to a maximum; but it appears more natural to say that, whenever x is very small, the differential formulæ cannot give the exact effects produced by dh and dh'.

I have to give an example of all these calculations.

Let us suppose, with Mr. Gauss, that $\delta=8^\circ\,22'\,43'',1$; $\delta'=28^\circ\,2'\,13'',4$; $\theta=62^\circ\,33'\,26'',9$; $h=h'=45^\circ\,44'\,52'',6$.

Calculation of the subsidiary Angle V according to Mr. Gauss.

Tang δ ..	9·7263516	tang θ	0·2845877
		cos F	9·8158318
C cos θ ..	0·3364323	C sin F $-\delta$	0·1852598
	0·0627839	tang V $=62^\circ\,36'\,58'',79$	0·2856793
tang F $=$	$49^\circ\;7'\;37'',70$		
δ	8 22 43 ,10		
F $-\delta=$	40 44 54 ,60.		

Trigo-

Trigonometrical Method.

Cot θ	0·2736484	tang θ..........	0·2845877
Cos θ	9·6635677	sin u	9·8158318
Ta.$x=40^\circ 52' 22'',30$	$=9·9372161$		
$\delta=$ 8 22 43 ,10		C cos $(\delta+x)$....	0·1852598
$\delta+x=$ 49 15 5 , 4		tan V 62°36′58″,79	0·2856793

Both methods require seven logarithms, which are either the same as in the second operation, or arithmetic complements of one another, as in the first. F and x, F $-\ \delta$ and $\delta+x$ are arcs complements of each other.

On the contrary, the operations required in order to find out the third auxiliary angle are quite different.

Calculation of the Angle W according to Mr. Gauss.

Sin F	9·8786157	$n-1=1·123433$......	0 3270489
C sin δ'	0·3278629	cos V	9·6627075
C cos (F$-\delta$)	0·1205703	tang h	0·0113399
		cot (F$-\delta$)	0 0646895
Sin h.......	....	cos W $=$ 52° 0′ 14″,25	9·7893035
C sin h.....	....	V $=$ 62 36 58 ,79	
$n=2·123433$	0·3270489	V$-$W$=u=$ 10 36 44 ,54	

This operation requires nine logarithms in the case of $h=h'$; it would require eleven, if the altitudes were unequal.

In the case of both being equal, the trigonometrical method would have much the advantage. I neglect it in order to give calculation the disposition requisite for all cases.

Trigonometrical Calculation.

Cos V............	9·6627075	$n-1=1·123433$..	0·0505666
Tang $(\delta+\lambda)$	0·0646895	Tang h..........	0 0113399
Cot D$=61^\circ 54' 20',5$	9·7273970	Cot D............	9·7273970
C cos D	0·3270490	cos W$=52^\circ 0' 14'',25$	9·7893035
C sin h			
Sin.. h'			
$n=2·123433=$	0·327090		

I must first find cot D $=$ cos V tang $(\delta+x)$. The logarithms of cos V and tang $(\delta+x)=$ cos (F$-\delta$) are common to both methods. The arc D is the third side of the triangle which has given V.

In the case of both being equal, the cosine of D is the number n of the analytical method. Commonly $n=\frac{\cos D \sin h}{\sin h'}$; $n-1$ is common to both methods, they assign a same value to the angle W.

The trigonometrical method only requires eight logarithms

garithms when $h = h'$, and ten when the altitudes are unequal.

In the case of two equal heights, we would have cos W = tang h tang $\frac{1}{2}$ D, and this angle would not require more than six logarithms instead of nine.

Here is the calculation of it:

Tang h	0·0113399	W = 52° 0′ 14″,25
Tang $\frac{1}{2}$D = 30° 57′ 10″,25	9·7779636	V = 62 36 58 ,79
Cos W 52 0 14 ,25	9·7813035	u = 10 36 44 ,54

The same simplification could be obtained from the analytical formula; but it could not be so easily perceived. Thus analysis, handled as it should be, conduces to the same solution as spherical trigonometry; but it is not always easy to choose such modifications as would reduce to the least terms the calculation of a complicated formula.

Analytical Calculation of the Horary Angle.

C cos u	0·0074926	Tang u.........	9·2726964
Tang h	0·0113399	Cos G	9·8398647
Tan G. . 46° 14′ 30″,77	0·0188325	C sin (G − δ) ..	0·2119881
δ = 8 22 43 , 1		Tan λ = 11° 55′ 18″,31	9·3245492
G − δ = 37 51 47 ,67			

Trigonometrical Calculation.

Cos u	9·9925074	Tang u.........	9·2726964
Cot h	9·9880061	Sin z..........	9·8398647
Tang z = 43° 45′ 29″, 2	9·9811675	C cos ($\delta + z$)....	0·2119881
δ = 8 22 43 ,10		Tan λ = 11° 55′ 18″,31	9·3245592
$\delta + z$ = 52 8 12,33			

Which shows that both operations are identical, which also is the case of the following:

Calculation of the Latitude.

Analytic Method.		Trigonometric Method.	
Cos λ............	9·9905300	Cos λ............	9·9905300
Cot (G − δ)	0·1093281	Tang ($\delta + z$)	0·1093281
Tan φ 51° 31′ 47″,19	0·0998581	Tan φ 51° 31′ 47″,19	0·0998581

The analytical method requires twenty-five logarithms in the case of $h = h'$, and twenty-seven in the more general case.

The trigonometrical method would require twenty-four logarithms in the case of $h = h'$, setting aside the facility offered by the isosceles triangle. It requires twenty-six, in the more common case. In fact it only requires twenty-two in the case of two equal altitudes, which, therefore, has the advantage by three logarithms. It is true that the analytical method

method may be reduced to the same conciseness; in which case both methods would be perfectly identical: but the trigonometrical method will always have this advantage,—that it gives the formulæ at sight, while analysis cannot attain to them but by going a great round about, whereby one is apt to be misled.

The differential formulæ remain to be compared: but without these formulæ, leaving h and h' indeterminate, the errors $d\lambda$ and $d\varphi$ that will result from dh and dh' may be ascertained while calculating λ and φ.

In the first place it is evident that the errors dh and dh' can have no effect on x, V nor D: they only begin to have any influence on W.

By calculating W, I see that the logarithm of n will increase 20·5 parts for each second of error in dh', and will lessen as much for each second of error in dh.

Let us then suppose that h and h' are too great, the logarithm of n will be too great by $-20{,}5\ dh + 20{,}5\ dh'$, n will be too great of itself by $-0\ 00001 dh + 0{\cdot}00001 dh'$; for by any uncommon coincidence, n increases 0·00001 in the same manner as h increases $1''$, when the logarithm is increased 20·5 parts; $n-1$ will, therefore, be too great by $-0{\cdot}00001\ dh + 0{\cdot}00001\ dh'$.

The logarithm $(n-1)$ will therefore be too great by $-38{,}7\ dh + 38{,}7\ dh'$.

The logarithm of tang h will be too great by $42{,}1\ dh$.

The logarithm of cos W will be too great by $+3{,}4\ dh + 38{,}7\ dh'$; but W decreased 26·9 part for $1''$. W then is too small by $\frac{3{,}4}{26{,}9} dh + \frac{38{,}7}{26{,}9}\ dh$, or by $0{\cdot}1264\ dh + 1{\cdot}4387\ dh$; u is then too great by the same quantity, since $u = \text{V} - \text{W}$.

Thus, by supposing that $dh = dh' = 10''$, u will be too great by $15''{,}65 = 1{\cdot}264 + 14{\cdot}387$.

Cos u will be too little by $0{\cdot}5056\ dh + \ 5{\cdot}7548\ dh'$.
Cot h will be too little by $\quad 42{,}1\ dh$

Tang z too little by ...... $42{\cdot}6036\ dh + 5{\cdot}7548\ dh'$.
z too little by ...... $1{\cdot}0096\ dh + 0{\cdot}1364\ dh'$.
$\delta + z$ too little likewise by the same amount.

Tang u will be too great by $+14{\cdot}700\ dh + 167{\cdot}320\ dh'$.
Sin z too long by ...... $-22{\cdot}212\ dh - \ 2{\cdot}864\ dh'$.
C cos $(\delta + z)$ too long by $-27{\cdot}361\ dh - \ 3{\cdot}696\ dh'$.

Tang λ too great by ..... $-34{\cdot}873\ dh + 160{\cdot}760\ dh'$.
λ too great by ..... $-0{\cdot}3350\ dh + 1{\cdot}5444\ dh'$.

Thus, supposing $dh = dh' = 10''$, $d\lambda$ will be $+ 12''094$, the horary angle too great by $12''$.

Cos

Cos λ too great by	$+1{\cdot}474\,dh - 6{\cdot}7954\,dh'$.
Tang $(\delta+z)$ too great by	$-43{\cdot}919\,dh - 5{\cdot}9322\,dh'$.
Tang u ϕ too great by	$-42{\cdot}445\,dh - 12{\cdot}7276\,dh'$.
ϕ too great by	$-0{\cdot}9803\,dh - 0{\cdot}2939\,dh'$.

And, supposing $dh = dh' = 10''$, the latitude will be too little by $12''78$: by recommencing the calculation and using the two altitudes as increased $10''$, I had found $d\lambda = +12''$ and $d\phi = -13''$.

This method of logarithmic differentials is general; it is more concise than the direct calculation with increased altitudes, but it requires a very minute attention. The formulæ are more commodious. I begin with mine.

Cot D	9·7274	Tang h	0·0113..	C sin D	0·0544
C sin W	0·1035	Cot W	9·8927..	C sin W	0·1035
+0·6775	9·8309	0·8017	9·9040..		0·1579
−0·8017					

$-0{\cdot}1242\,dh - 1{\cdot}438\,dh' = -du$. Let us suppose $dh = dh' = 10''$, we shall have $du + 0{\cdot}1242\,dh + 1{\cdot}438\,dh' = 15{\cdot}622$: the logarithmic variations have given $15'',65$.

Cot u ..	0·7273	−sin δ ..	−9·1635	−cos δ ..	−9·9953
Cos λ ...	9·9905	sin λ ..	9·3151	$\sin^2\lambda$	8·6302
+5·2216	0·7178	−0·0301	8·4786	C $\cos^2 h$	0·3126
−0·0301				C sin u	0·7348
+5·1915	0·7153	....	0·7153		9·0729
Sin λ ..	9·3151	....	9·3151		$-0{\cdot}4709\,dh'$
0·1242	9·0941	1·438	0·1579		$+1{\cdot}5427\,dh'$
$+0{\cdot}1333\,dh$	9·1245	$+1{\cdot}5427\,dh'$	0·1883		$+1{\cdot}0718\,dh$

$d\lambda = +0{\cdot}133\,dh + 1{\cdot}0718\,dh'$.

Suppose $dh = du = 10''$, we shall have $d\lambda = +12'',05$, the logarithmic variations have given $12'',09$.

C cos ϕ	0·2061	0·2061	....		−0·2061
Cos h ..	9·8437	−sin h	−9·8552	cos h	9·8437
Sin δ ..	9·1635	cos δ	9·9953	cos δ	9·9953
		cos u	9·9925	sin u	9·2652
+0·1634..	9·2133	−1·1197	0·0491	−0·2042	−9·3103
		+0·1634		0·1242	9·0941
		$-0{\cdot}9563\,dh$		−0·0254	8·4044
		$-0{\cdot}0254\,dh$			

$$d\phi = \begin{cases} -0{\cdot}9817\,dh \\ -0{\cdot}2939\,dh' \end{cases}$$

−0·2042	−9·3103
1·438	0·1579
−0·2939	−9·4682

Supposing $dh = dh' = 10''$, $d\phi = -12'',756$. The logarithmic differentials have given $-12''78$. A more satisfactory coincidence cannot be wished for. These formulæ appear to require more calculation than the logarithmic differences, but they do not require so minute an attention.

Mr. Gauss's

Mr. Gauss's formulæ are simpler, but they suppose that the two azimuths are determined, in order to which it is sometimes necessary to make use of the longest formulæ, to avoid all uncertainty on the description of the angle.

For the greater uniformity and generality, I chose the following formula:

$$\text{Cot A} = \frac{\tan\delta \cos\phi - \sin\phi \cos\lambda}{\sin\lambda}$$

Tang δ	9·1683	$-\sin\phi$	$-9{\cdot}8937$
Cos ϕ	9·7939	$\cos\lambda$	9·9905
$+0{\cdot}09166$	8·9622	$-0{\cdot}76594$	$-9{\cdot}8842$
$-0{\cdot}76594$			
$-0{\cdot}67428$	$-9{\cdot}8288$ negative denominator.		

C sin λ $+0{\cdot}6849$ positive denominator.

Cot A $= (162^\circ\ 57'\ 50'')$ 0·3137 A must be in the second quadrant.

We shall obtain the other azimuth A′ by a similar formula.

Tang δ	9·7263	$-\sin\phi$	$-9{\cdot}8937$
Cos ϕ	9·7939	$\cos(\lambda-\theta)$.	9·8023
$+0{\cdot}33115$	9·5202	$-0{\cdot}49660$	$-9{\cdot}6960$
$-0{\cdot}49660$			
$-0{\cdot}16545$	$-9{\cdot}2187$ denominator negative.		
C sin $(\lambda-\theta)$	$-0{\cdot}1118$ numerator negative.		

Cot A′ $= 257^\circ\ 55'\ 10''$ $+9{\cdot}3305$ A′ will be in the 3d quadrant.

	162 57 10			
A′ − A	94 57 20 ...	0·0016	C sin (A′ − A)	0·0016
		0·2601	C cos ϕ	0·2601
	$+\cos$ A′	$-9{\cdot}3208$	$-\cos$ A $+$	9·9805
	$-0{\cdot}3824dh$	9·5825	$+1{\cdot}7467dh'$	0·2422

$\delta\lambda = -0{\cdot}3824dh + 1{\cdot}7467dh' = +13{\cdot}643$, in supposing $dh = dh' = 10''$.

	0·0016	C sin (A′ − A)	0·0016
$+\sin$ A′ .	$-9{\cdot}9903$	$-\sin$ A	$-9{\cdot}4668$
$-0{\cdot}9815dh$	$-9{\cdot}9919$	$-0{\cdot}2940dh'$	9·4684

$d\phi = 0{\cdot}9815dh - 0{\cdot}2940dh' = -12'',7555$, supposing $dh = dh' = 10''$.

The azimuths are computed from the inferior meridian, which I suppose to be 0 to 360°, eastward. The λ are positive on the east side, and negative on the west. They are of 180° at the lower meridian, 360° or 0 at the upper.

The differential expressions $dh = d\phi \cos A - d\lambda \cos\phi \sin A$

$dh' = d\phi \cos A' - d\lambda \cos\phi \sin A'$

which Mr. Gauss has given, and may be easily deduced from my formulæ, might be still more easily demonstrated by the figure which has supplied the fundamental equation of the problem: they show evidently that if $A' = A$, dh will necessarily be $= dh'$; for the second members being equal, the first must be so likewise. If $A' = 180' + A$, dh' will be equal to $-dh$. In fact, in the first case, $h' = h + d$, and in the second case $h + h' + D = 180$. Thus, if the first altitude h had been observed, it would only be required to set down the time when h' would be equal to $h + D$, or where $h' = 180 - h - D$; but it would be necessary to be assured that the second star would pass by the same vertical as the first: but if two complete observations of altitudes and times are made, it is possible that dh' and dh will be such that the altitudes h and h' will no longer agree with the time elapsed, with the two declinations and with the distance D. Thus, the suppositions it is necessary to admit no longer agree with what passes in the heavens; and in order that the differential formulæ could be established, either the declinations or the angle θ should have been made to vary.

If A' differs not much from A, the two equations will not differ much from each other. And as it is from their differences that the values of $d\varphi$ and $d\lambda$ are computed, in order properly to determine those two quantities, an extreme precision is requisite in ascertaining the values of A' and A, a precision which can be expected neither from the observation nor the calculation. These differential formulæ are therefore only approximations, on which dependence should not always be placed, which, however, does not imply that $d\varphi$ and $d\lambda$ will in that case be very considerable. If the cosines of the azimuths are small, it may be expected that the latitude will not be well determined; if the sines of the azimuths are small, the hour will not be well known; but if one of the observations is made towards the first vertical, and the other towards the meridian, all the precision will be obtained that can be expected from this method, which may be of great value, when no other is feasible, but which can never be compared with those that separately determine the latitude and the time.

Mr. Van Beck Calkoen, director of the observatory royal at Utrecht, has just given a different solution of this problem, with a slight change, which was not even indispensable. It consists in reducing to the same moment the observations of the two altitudes. For this purpose Mr. Calkoen commences by observing one of the two stars immediately

immediately after he observes the other, and after that he again observes the first;—few minutes are sufficient for these three observations. During this interval, it may be supposed that the first star has raised by an uniform motion; and by means of simple proportional parts, the altitude of the first star is reduced to the moment when the second was observed.

This being laid down, we shall, in order to save a figure, express in general formulæ Mr. Calkoen's solution.

Let D be the distance between the two stars, we shall have $\cos D = \cos(AR - AR') \cos\delta \cos\delta' + \sin\delta \sin\delta' \ldots (1)$.

Make $\cot E = \dfrac{\sin\delta' - \sin\delta' \cos D}{\sin\delta \sin D} = \left(\dfrac{\sin\delta'}{\sin\delta} - \cos D\right) \dfrac{1}{\sin D} \ldots\ldots (2)$.

E will be the continuation of the distance D till it reaches the equator (line.)

$\text{Sin}\ A = \dfrac{\sin\delta}{\sin E} \ldots\ldots (3)$.

A will be the angle by which the continuation E intersects the equator.

Make $\cot F = \dfrac{\sin h' - \sin h \cos D}{\sin h \sin D} = \left(\dfrac{\sin h'}{\sin h} - \cos D\right) \dfrac{1}{\sin D} \ldots\ldots (4)$.

F will be the continuation of the distance D to the horizon.

$\text{Sin}\ B = \dfrac{\sin h}{\sin F} \ldots\ldots (5)$.

B will be the angle by which the continuation F will intersect the horizon. E and F will be in the same plane.

Make $\tan x = \cos(E - F) \tan B \ldots\ldots (6)$.

$\text{Sin}\ \varphi = \dfrac{\cos B \cos(A - x)}{\cos x} \ldots\ldots (7)$.

Then $(90^\circ - \varphi)$, $(90 - \delta)$, and $(90 - h)$ will be the three sides of a spherical triangle, and the angle opposite to $(90 - h)$ will be the horary angle of the star whose declination is δ.

Thus eight formulæ give the solution of the problem, which is founded on a simple and ingenious construction. The demonstration of the formulæ is easy to find, it is perceived on the simple inspection of the figure. The solution requires 33 logarithms actually different; or, with tables of natural and logarithmic sines, 30 only; but in all cases it is longer than the preceding solution.

The advantange that could be found by it is, that for two given stars the arc of distance D is almost constant, as well as its continuation E, and the angle A on the equator. These three quantities vary but slowly by the procession, aberration, and nutation, which are a little different in the two stars.

Tables of them could, therefore, be made which would save about 16 logarithms, or half the work. These tables could, without sensible error, be used for some weeks; but this advantage would be lost, if the problem was restored to its original form, by not taking the first altitude at the same moment as the second. Then, instead of $(Æ - Æ')$, $(Æ - Æ' - m)$ must be employed.

The distance D would no longer be the real distance between the two stars. The remainder of the solution would not be altered; but as it is simple and natural, it would be better to substitute our formulæ.

For the ascertaining the value of the error that is to be apprehended in the latitude φ, Mr. Calkoen gives the same formula as Mr. Gauss; he does not give the one for the horary angle.

Mr. Gauss's method was no more than the vulgar method expressed and demonstrated by analysis simply: Mr. Calkoen's is solely founded on a geometrical construction, which we have reduced to general formulæ. Dr. Mollweyde has just given in Mr. de Zach's Journal of June 1809, p. 545, a purely analytical method, which has no relation with the trigonometrical solution.

Like Mr. Gauss, he commences by the equation $\sin h = \sin \delta \sin \varphi + \cos \delta \cos \varphi \cos \lambda$; and $\sin h' =$, &c.

He makes $z = \text{tang}\,(45^\circ - \frac{1}{2}\varphi)$ $\sin \varphi\ \frac{1-z^2}{1+z^2}$, $\cos \varphi = \frac{2z}{1+z^2}$; he substitutes those values, and uniting all the known quantities which he designs by M, N, M′, N′, he derives the two equations $M + Nz^2 = z \cos \lambda$

$M' + N'z^2 = z \cos (\lambda - \theta)$; he cuts off z^2 and finds $z = \frac{M' N - M' N'}{N \sin \theta \sin \lambda - (N' - N \cos \theta) \cos \lambda}$; after which, by substitutions too extensive to detail here, he obtains the following formulæ:

$$\text{Tang}\,(45^\circ + A) = \frac{\sin \frac{1}{2}(h' + \delta') \cos \frac{1}{2}(h' - \delta') \cos \delta}{\sin \frac{1}{2}(h + \delta) \cos \frac{1}{2}(h - \delta) \cos \delta'};$$

$$\text{Tang}\,(45^\circ + A') = \frac{\cos \frac{1}{2}(h' + \delta') \cos \frac{1}{2}(h' - \delta') \cos \delta}{\cos \frac{1}{2}(h + \delta) \sin \frac{1}{2}(h - \delta) \cos \delta'};$$

Tang

$$\text{Tang } B = \text{tang } A \cot \tfrac{1}{2}\theta. \quad \text{Tang } B' = \text{tang } A' \cot \tfrac{1}{2}\theta.$$
$$C = (B' + B + \theta) \text{ and } D = (B' - B).$$
$$\text{Tang } E = \frac{\sin (h + \delta) \sin (h - \delta) \sin D}{2 \cos^2 \delta \cos \tfrac{1}{2} (C + D) \cos \tfrac{1}{2} (C - D)};$$
$$\text{Cos } (2\lambda - C) = \frac{\cos (D - E)}{\cos E} = \cos (C - 2\lambda).$$
$$\text{Tang } (\lambda - \tfrac{1}{2} C) = \pm \surd\ (\text{tang } \tfrac{1}{2} D \text{ tang } \tfrac{1}{2} (D - E).$$
$$\text{Tang } (45° - \tfrac{1}{2} \phi) = \frac{\cos \tfrac{1}{2} (h + \delta) \sin \tfrac{1}{2} (h - \delta) \sin D}{\cos \delta \sin \left(\lambda - \frac{C - D}{2}\right) \cos \tfrac{1}{2} (C + D)}.$$

Of the two values of λ, the one is chosen which makes ($45° - \frac{1}{2}\phi$) $\angle$ $45°$; that is to say, ϕ positive. Those formulæ are purely analytical, at least it is difficult to perceive in what they could assimilate to the ordinary solution. They are elegant; they indicate, in a more direct manner than any of the preceding methods, the choice to be made between the two roots of the equation, but at the same time require too long a preparation, more logarithms, and have besides that great inconveniency, in a method of common use, that it cannot be retained by the best memory. This solution requires 29 logarithms absolutely different; besides that ten of these logarithms are to be used twice; so that, with all the compositions of half sums and half differences, the operation is longer by half than that of our formulæ: thus, notwithstanding its elegance and general application, I do not expect it will be preferred in practice.

XXXIV. *On De Luc's Electric Column.* *By Mr.* B. M. Forster.

To Mr. Tilloch.

Walthamstow, 16th March 1811.

Sir, Since my Letter, on *A Method of fitting up, &c. the Electric Column, invented by Mr. De Luc*, was published in your Magazine for March 1810, *Mr. De Luc* has sent a series of letters to *Nicholson's Philosophical Journal*, on the *Galvanic Pile* and on his *Electric Column*, in which are several very interesting and curious particulars; a few of which I shall here relate.

He made what he calls *dissections* of the *pile*, by means of *tripods* of brass wire. The first dissection was by placing the *tripods* between each plate of zinc and silver, thus separating *groups* in which the metals had *between* them *wet cloth*. With this pile, the effects appeared to be the same as in

in a common pile, both electrical and chemical (as Mr. De Luc terms them) being produced.

In the second *dissection*, the *tripods* separated *groups* composed of the two *metals* in contact, and the *wet cloth* in contact only with the *zinc* plates. In this were produced *electric* effects but not *chemical*; no extrication of *gas* or *calcination* of wire was perceived in a glass tube filled with water placed between the extremities of this pile, but the electroscopes at each end showed electric signs. When the cloth was wetted with a solution of marine salt, the *shock* was produced both in this and the first dissection.

In the third *dissection* the *tripods* divided the *groups* composed of the two *metals* in contact; but the *wet cloth*, in contact only with *silver*. In this experiment no *electric* or *chemical* effects were produced.

Mr. De Luc mounted a pile of 76 groups of *zinc* and *silver*, but instead of *wet* cloth used new cloth, which had stood some time in his room, his *hygrometer* being about 40°. Electric signs were produced, but weaker than when *wet* cloth was used. When the *glass tubes* were applied, there was no appearance of *chemical effects*. He afterwards substituted *writing-paper* for cloth; the *electric* signs were increased, but no *chemical* effects were produced. After various trials with different metals, he formed piles of zinc and Dutch-gilt paper, connecting the plates with a silken thread, in the form of *chaplets*, which *chaplets* he inserted in glass tubes, with metallic caps and screws, much like, as appears from the description, to the *columns* or tubes, which in my letter I have suggested might be called *Electric Rods*. This mode of fitting them up was adopted before mine were made.

To the third communication from him is a figure of the *Electric Column*, mounted on glass pillars, with *Electroscopes* and other parts of his apparatus. The whole series of these papers, if collected in one volume, would form a valuable addition to the books already published on Electricity.

The bells which I mentioned were placed in communication with three (Electric) *Rods* on the 14th of March 1810, continued to ring until the 24th, on which day I could not hear them for about a minute; after which they rang again. The next day I also thought they ceased for a short time, but have great doubts whether I was not mistaken: from that day (the 25th of March) they did not, to my knowledge, once cease until the 4th of September, a period of 163 days. On the 4th, at about ten o'clock at night,

night, for about ten minutes I did not hear them, but believe they were ringing before that time, and stopped whilst I was in the room: afterwards the same night they made several short pauses, perhaps for the space of a second or less. This kind of cessations I frequently noticed in the months of September and October: on the 16th of the latter they were ringing in the morning, with pauses, but in the evening I found they had stopped: from this time until the 25th, no ringing; about nine o'clock on that morning they again began (not having been touched), occasionally making short pauses as before, and continued ringing in this manner until the 16th of November, on which day they twice, for very short periods, rang: the ringings I *suspected* might be owing to some accidental shake of the apparatus, which occasioned the clapper to be released from one of the bells, to which it *perhaps* was attracted. On the 17th, in the evening, they once rang, owing again I thought to a shake. On the 18th of November, not having heard them ring, I disunited them from the columns. I do not wish it to be understood that I give the above statement as perfectly certain, for it is very probable that the bells might cease and ring at times when no observations were made.

In my paper I have miscalled the ends of the columns, so that the experiment of the charged coated jar was exactly what might have been expected from the known effects of the Voltaic Pile.

On reading *Volta's* account of the Galvanic Pile in the *Philosophical Transactions*, it appears the idea of forming an artificial Electric Eel, by adding a head and tail to the pile, had occurred to him, which I was not aware of when I gave you the description of one in my letter.

It is a remarkable circumstance, that although these columns act powerfully on electrometers, and sufficiently strong to keep in motion a small brass ball, yet with 1500 series of plates there did not appear to be any production of gas or calcination of wire in a glass tube filled with water, placed between the two ends of the combined columns,—whereas these effects are visible in a *Voltaic Pile* consisting of a series of 25 plates of the size of a half-crown. I have some rods fitted up with caps which have not any screws, and provided the tubes are so filled as to cause a sufficient degree of pressure (much is not necessary), there does not appear to be any need of screws. Ivory caps for this purpose serve very well, *perhaps* not quite so well as brass. Columns of about 450 series of the small-

sized plates (about a quarter of an inch diameter) in tubes with ivory caps, make very neat pocket instruments, and are sufficiently powerful to move a small ivory needle suspended on a point. I remain, &c.

B. M. FORSTER.

XXXV. *Memoir on the Connexion which exists between the Oxidation of the Metals, and their Capacity of Saturation for the Acids. By M.* GAY LUSSAC *.

It has been long known that there are some metals which precipitate other metals from their acid solutions in the metallic state. Bergman had remarked, expressing himself according to the chemical theory then adopted, that the phlogiston of the precipitating acted on the precipitated metal, and Messrs. Sylvester and Grothuis have besides shown, that when precipitation has once commenced by a chemical affinity, it may continue by a process purely Galvanic. Nevertehless the chief phænomena which accompany this precipitation have escaped the attention of chemists. We are ignorant, in particular, of the relation which subsists between the quantity of the metal precipitated and that of the precipitating metal; and of course we are ignorant of the important consequences which flow from it. I propose to determine this relation, and to prove that the quantity of acid which the various metals require to saturate them is in a direct ratio to the quantity of oxygen which they contain. I have arrived at this principle, not by the comparison of the known proportions of the metallic salts, which are in general not so very exact as to enable us to recognise any law, but by observing the mutual precipitation of the metals from their acid solutions.

When we precipitate in fact a solution of acetate of lead by a piece of zinc, a beautiful vegetation is formed known by the name of *arbor Saturni,* and which is owing to the reduction of the lead by a Galvanic process. We obtain at the same time a solution of acetate of zinc equally neutral with that of lead, and entirely exempt from this last metal. Little or no hydrogen is extricated during the precipitation; which proves that the whole of the oxygen necessary to the zinc for dissolving and saturating the acid has been furnished to it by the lead.

If we put into a solution of sulphate of copper slightly

* *Mem. d'Arcueil,* tome ii. p. 159.

acid

acid some well cleaned iron filings, and in a large proportion, the copper is precipitated almost instantaneously, the temperature is considerably raised, and no gas is extricated. The sulphate of iron which we obtain is that in which the oxide is at the minimum, and its acidity is precisely the same with that of the sulphate of copper employed.

We obtain similar results by decomposing the acetate of copper by lead, particularly by the help of heat. But since zinc precipitates lead from its acetic solution, we must conclude that it would also precipitate copper from its combination with acetic acid. Experience in this case accurately agrees with theory.

We know with what facility copper precipitates silver from its nitric solution. The whole oxygen which it requires to dissolve it is furnished by the oxide of silver; for no gas is extricated, and the acidity does not change.

It is also the same case with copper as with nitrate of mercury, and with cobalt with respect to the nitrate of silver.

In these last examples, as in the foregoing, the precipitating metal finds in the oxide of the metal which it precipitates, the whole of the oxygen which is necessary to oxidize it, and to neutralize to the same degree the acid of the solution.

These facts, which appear to me to be incontestable, naturally lead to the principle which I have given out, namely, that the acid in the metallic salts is directly in proportion to the oxygen in their oxides. For since the precipitating metal finds in that which is precipitated all the oxygen which it requires to oxidate it, and to neutralize to the same degree the acid of the solution, it follows that the quantity of oxygen in every oxide remains the same; and consequently the less of the precipitating metal is dissolved, the more affinity it has for oxygen. Thus if we suppose two metals, one of which takes twice as much oxygen as the other, it will dissolve twice as much of the latter as of the former, in order to neutralize the same quantity of acid.

It is therefore proved, that when zinc, iron, lead, copper, silver, cobalt, and mercury are precipitated from their solutions in the metallic state, the precipitating metal finds in that which is precipitated all the oxygen necessary to oxidize it, and to neutralize to the same degree the acid of the solution. The other metals ought also to enjoy the same property; but there are some circumstances which I shall make known, which render it difficult to prove it.

If

If we precipitate the muriate of antimony by zinc, we obtain a compound effect. This salt being with a very great excess of acid, and the muriate of zinc being perhaps almost in the neutral state, it happens at the same time that the zinc takes up the oxygen from the antimony, it decomposes the water, and consequently produces hydrogen gas. It may also happen that the salt to be precipitated is neutral, and that which we ought to obtain does not enjoy this property: in this case, the metal precipitated is mixed with more or less oxide. Finally, the precipitating metal may exercise a very powerful action on the acid of the solution, may decompose it, and give a complex product. This happens when we precipitate by zinc the nitrate of copper*: a part of this metal decomposes a part of the acid, and the oxide which results from it precipitates oxide of copper which is mixed with the metallic copper. But these particular facts are not opposed to the principle which I have established, and we could give a very satisfactory explanation of it.

The oxides which I have hitherto considered are in general those which M. Proust has called *oxides at the minimum*, and we may ask if those which are *at the maximum* enjoy the same properties: *i. e.* if their capacity of saturation for the acids is proportional to the quantity of oxygen which they contain.

It would seem that we ought not to have any doubt on this head; for nothing indicates *a priori*, if the oxides which we regard as being at the *minimum* are effectually so; and even when they should be at the *minimum* or at the *maximum*, relatively to the chemical means which we employ to produce them, it does not follow that they would represent corresponding degrees of oxidation. But without entering upon discussions which would perhaps be foreign to my object, I may quote some facts which give considerable generality to the principle which I have established. These facts, however, will not be very numerous; for almost all the metals, when they are very much oxidized, form salts with a greater or less excess of acid which is foreign to the saturation, and which has no other effect than to overcome the cohesion of the oxide and to hinder its precipitation.

We may in the first place prove that the highly oxidated metals take more acid than those which are less so. M. Proust

* Vauquelin, *Ann. de Chimie*, tome xxviii. p. 45.

Proust has observed that mercury kept for some time in a solution of corrosive sublimate is changed into mild muriate, and Messrs. Fourcroy and Thenard have found by the analysis of these two mercurial salts that the former contains more acid than the latter. In the same way, when we expose to the air the white muriate of copper, it is changed into green muriate and into oxide retaining a little acid; an evident proof that the copper takes more acid in proportion as it is more oxidated. But it is not enough to know that the highly oxidated metals take more acid than those which are less so: it is necessary to show that they take the acid precisely in the ratio of their oxidation.

By repeating several times the distillation of mercury with its oxidized muriate, the whole salt is changed into mild muriate, and neither acid nor oxygen gas is extricated. It is therefore evident that there are in the oxidated muriate, and in the mild muriate, quantities of acid proportional to the quantities of oxygen in the mercury. The analysis of these two salts also leads to the same conclusion. Messrs. Fourcroy and Thenard found that the oxidated muriate contains 73 parts of mercury and 20 of acid, and the mild muriate 11·6 of acid and 83 of mercury. They moreover found that the red oxide contains twice as much oxygen as the black oxide; so that by making the calculation, in order to bring the metal to the same weight in both analyses, we see that the acid in the sublimate and the mild muriate is in the same ratio with the oxygen in the oxides of each salt. By comparing under the same point of view the analyses of the muriates of copper given by M. Proust and Mr. Chenevix, we also obtain the same results.

I am of opinion, therefore, that we may fairly conclude from these facts, that the acid in the salts is exactly proportional to the oxygen of the oxides*.

We see already as a consequence of this principle, that by decomposing a neutral metallic salt by a metal susceptible of also forming a neutral salt, neither oxygen nor acid ought to be extricated, considering that both are in a ratio proper for forming a neutral salt with the decomposing metal.

But the most important result which we can draw from the principle that the acid in the salts is proportional to

* It is very remarkable that, the acid being proportional to the oxygen of the oxide, the latter does not follow the ratio of the oxygen when the acid passes from the first to the second degree of acidification. A sulphite, that of lead for instance, would take more acid if the metal were more oxidated; but it would not take more oxide if the acid passed to the state of sulphuric acid. I am ignorant how this difference arises.

the

the oxygen, is a very simple method of determining the proportions of all the metallic salts. With the exception of the proportions of the insoluble salts, those of all the rest present an uncertainty more or less great; and it would be useful to make it disappear, by making the proportions of acid in the metallic salts depend on those of the oxygen contained in their oxides. It is sufficient, in fact, to know the various degrees of oxidation of the metals, to conclude from them the relative quantities of acid which they require to saturate themselves; and in order to have the absolute quantities, it is sufficient to know the proportions of any salt in each genus.

I shall take as a first example the muriates, and I shall suppose, on weighing the experiments of several chemists with mine, that the muriate of silver contains:

Silver	100·00
Oxygen	7·60
Acid	25·73

On the other hand,

100 of copper takes	24·57	oxygen	according to my experiments.
Zinc	24·41	———	
Lead	7·29	———	M. Berthier.
Mercury at the *minimum*	4·16	———	Messrs. Fourcroy and Thenard.
Ditto at the *maximum*	8·21	———	

If we admit these data, and the principle that the capacities of the metals for the acids are proportional to the quantity of oxygen which they contain, we find the following proportions for the muriates which are deprived of water:

$$\text{Muriate of lead} \ldots \begin{cases} 100{\cdot}00 \text{ lead.} \\ 7{\cdot}29 \text{ oxygen.} \\ 24{\cdot}68 \text{ acid.} \end{cases}$$

Mr. Kirwan has found for this salt 24·02 of acid. If we calculate the proportion of this last according to the analysis of the sulphate of lead by M. Berthier, and according to the relation of the capacities of the muriatic acid and sulphuric acid deduced from the analysis of the sulphate and the muriate of barytes, by the same chemist, we find 24·55; which agrees very exactly with the number 24·68 which I have given.

$$\text{Muriate of copper} \ldots \begin{cases} 100{\cdot}00 \text{ copper.} \\ 24{\cdot}57 \text{ oxygen.} \\ 83{\cdot}18 \text{ acid.} \end{cases}$$

Instead of this number M. Proust has found 74·74.

$$\text{White muriate of copper} \begin{cases} 100{\cdot}00 \text{ copper.} \\ 12{\cdot}28 \text{ oxygen.} \\ 41{\cdot}59 \text{ acid.} \end{cases}$$

Messrs.

Messrs. Proust and Chenevix found 39·5.

Muriate of mercury (sweet mercury)	100·00 mercury. 4·16 oxygen. 14·08 acid.

Messrs. Fourcroy and Thenard found 13·97.

Oxidated muriate of mercury (corrosive sublimate)	100·00 mercury. 8·21 oxygen. 28·16 acid.

The same chemists found 27·39.

By setting out from the proportions of the sulphate of lead given by M. Berthier, we easily find those of the other sulphates.

Sulphate of lead, according to M. Berthier	100·00 lead. 7·29 oxygen. 37·71 acid.
Sulphate of copper	100·00 copper. 24·57 oxygen. 127·09 acid.

Mr. Proust found 128·46.

Sulphate of zinc	100·00 zinc. 24·41 oxygen. 126·26 acid.

Mr. Tennant found 124·41.

If we wished to determine by experiments the proportions of the sulphites, we should experience great difficulties, because they are changed very easily into sulphates; but we may infer them from those of these last salts.

I have proved in another part of this volume of the Memoirs of the Society, that the sulphuric acid may be reduced by heat into two parts in volume of sulphurous gas and one of oxygen gas. We know, moreover, that a sulphite is changed into a sulphate without the neutrality changing. Consequently, the capacity of the sulphurous acid is to that of sulphuric acid, as the weight of two parts of sulphurous gas: plus one of oxygen gas is to that of two parts of sulphurous gas: *i. e.* by adopting the specific gravity of the oxygen gas by Lavoisier, and that of the sulphurous gas by Kirwan : : 2·583 : 2·076. By multiplying therefore the proportion of acid of each sulphate by this ratio, (0·8037) we shall have that of every corresponding sulphite.

It is thus that for the sulphite of lead we find	100·00 lead. 7·29 oxygen. 30·30 acid.

Lastly, in order to give still another example of the calculations to be made in order to determine the proportions of the salts of one and the same genus, I shall take the phosphates;

phates; and I shall admit with M. Berthier, that

The phosphate of lead is composed of $\begin{cases} 100\cdot00 \text{ lead.} \\ 7\cdot29 \text{ oxygen.} \\ 31\cdot14 \text{ acid.} \end{cases}$

According to this, we find by calculation the following proportions:

Phosphate of copper .... $\begin{cases} 100\cdot00 \text{ copper.} \\ 24\cdot57 \text{ oxygen.} \\ 104\cdot95 \text{ acid.} \end{cases}$

Phosphate of zinc .... $\begin{cases} 100\cdot00 \text{ zinc.} \\ 24\cdot41 \text{ oxygen.} \\ 104\cdot27 \text{ acid.} \end{cases}$

Phosphate of mercury at the minimum .... $\begin{cases} 100\cdot00 \text{ mercury.} \\ 4\cdot16 \text{ oxygen.} \\ 17\cdot76 \text{ acid.} \end{cases}$

Phosphate of silver ..... $\begin{cases} 100\cdot00 \text{ silver.} \\ 7\cdot60 \text{ oxygen.} \\ 32\cdot46 \text{ acid.} \end{cases}$

Since (the proportions of a metallic salt and the oxidation of the metals being given) we may determine those of all the salts of one and the same kind, we may also (the proportions of acid and oxide of all the metallic salts and the oxidation of a single metal being given) calculate that of all the rest. Thus, upon the supposition that the analysis of the sulphate of lead which I used is exact, and that of the sulphate of barytes (66·5 of barytes and 33·5 of acid) given by M. Berthier is correct also, we find that the new combustible substance extracted from barytes would take for 100 parts 10·77 of oxygen. In the same way, admitting the analysis of the muriate of soda which was communicated to me by M. d'Arcet, viz. 50·73 of alkali and 49·27 of acid, we should find that the new combustible substance extracted from soda takes 40·21 of oxygen: about four times more than the foregoing.

We know that lead, silver, and mercury at the *minimum* of oxidation form insoluble salts, with a very great number of acids. These metals are precisely those which form neutral salts, or nearly so, take less oxygen, and consequently less acid. We can easily conceive, therefore, how all the salts in which the oxide is at the minimum of oxidation, have more tendency to insolubility than those in which the oxide is at the *maximum*. It is a consequence of this general law, that when there is plenty of any insoluble principle in a compound, the latter has more tendency to insolubility than when it is a souble principle which prevails in it. The mercury at the minimum forms an insoluble salt; but when it is at the maximum, it gives

gives a salt which enjoys a great solubility, as well as all the metallic salts which take much oxygen.

I shall not extend this memoir further, as I think I have related a sufficient number of facts to establish the principle which is the object of it, and to make all its consequences apparent. The proportions of acid in the salts depending on the quantity of oxygen which the bases contain, it would be desirable if chemists would direct their attention, on the oxidation of the metals and on the proportions most easy to determine, to one or two salts in each genus. We should thus obtain the proportions of a great number of salts, and we should even have the advantage of calculating the limits of those of the acid salts, in proportion as they should approach neutrality more and more. For it must be well remarked, that the excess of acid in a salt is foreign to the saturation, and that it is only necessary to hinder the precipitation of the oxide by destroying its force of cohesion. If, in fact, the oxides were very soluble, they would all form perfectly neutral salts.

Observation.

When we precipitate a metallic solution by sulphuretted hydrogen alone, or combined with an alkaline base, we obtain a metallic sulphuret or hydro-sulphuret. In the first case, the hydrogen of the sulphuretted hydrogen is combined with all the oxygen of the oxide, and the sulphur forms a sulphuret with the metal. In the second case, the sulphuretted hydrogen is combined directly with the oxide without being decomposed, and its proportion is such that there is enough of hydrogen for saturating all the oxygen of the oxide. The quantity of the hydrogen destroyed, or capable of being so, depends therefore on the oxidation of the metal, in the same way as the quantity of sulphur which may be combined with it. Consequently the same metal forms as many distinct sulphurets as it is susceptible of degrees of oxidation in its acid solutions. And as these degrees of oxidation are fixed, we ought also to obtain sulphurets with constant proportions, which we may determine very easily, according to the quantity of oxygen of each metal and the proportions of the sulphuretted hydrogen. I do not pretend that these sulphurets are the only ones which we can obtain; but I think that we ought to regard them as the true types of the other sulphurets, so much the more as the proportion of the sulphur has an immediate relation with the quantity of oxygen which the metal had, and as the latter determines of itself the proportion of acid which is combined with it.

XXXVI. *A con-*

XXXVI. *A concise Description of Schooley's Mountain, in New-Jersey, with some Experiments on the Water of its Chalybeate Spring. By* Samuel L. Mitchill, *Professor of Natural History in the University of New-York, Representative in the Congress of the United States, &c. &c. Communicated by the Author.*

There had been so much conversation about Schooley's mountain, that in the beginning of July 1810 I executed the desire I had long entertained of visiting it.

Schooley's mountain is part of a chain which extends in a north-easterly and south-westerly direction across the state of New-Jersey. It may be traced from the Highlands of New-York. Towards the Hudson, its ridges divide the plains of Rockland county from those of Orange, being denominated the Haverstraw, Warwick, Skunemunk, and Stirling mountains, and being distinguished locally by several other names. Towards the Delaware, it separates the upper waters of the Raritan from those of the Musconetcunck, and passing from Sussex through Morris and Hunterdon counties, is called, somewhat to the southward of Philipsburg, the Musconetcunck mountain. The more noted portion of its middle region is termed Schuyl's Hills, or Schooley's Mountain. The latter name is the most prevalent, and is derived from a family which was formerly a considerable proprietor of the soil thereabout. The former appellation is probably a mere abbreviation or corruption of it.

This ridge discharges the water from its north-west side, partly through the Wallkill, into the Hudson, a little to the eastward of Esopus, after traversing Sussex county, in New-Jersey, and Orange and Ulster, in New-York. Part also empties into the Hudson through Murderer's Creek, at New-Windsor. Another portion is collected into the Musconetcunck river; and running almost parallel with the mountain, falls into the Delaware, not many miles south of Eastown. The water from the south-east side feeds the upper streams of the Pasaick, which, after visiting Orange, Rockland, Morris, Essex, and Bergen counties, falls into Staten Island sound, to the southward of Newark. The stream called the Black river beyond Mendham, and that termed South-branch, watering Dutch valley, neither of them reach the Delaware, but empty into the Raritan, some distance above Brunswick.

Thus these heights completely divide the waters of New-Jersey.

Jersey. Not a single stream is known to pierce them. From their north-western slope, all their streams find their way into the Hudson and the Delaware. From their south-eastern declivity, their currents travel to the ocean by Newark and Raritan bays. They have, however, no pretensions to be classed with the Shawangunk mountains, which are a distinct chain, and make part of the great Alleghany, that traverses the continent to the confines of Georgia. Nor have they any connexion with the Kaatskill mountains, which are themselves quite detached from the Shawangunk. Schooley's mountain is of more moderate elevation than either. Geometrical measurement has ascertained that the height of Schooley's mountain above its immediate base is more than six hundred feet. And a calculation made by approximation, on the falls of water at the different mill-dams along the hurrying channel of the Musconetcunck, to its junction with the Delaware, and on the descent thence to Trenton, makes the position of that base to be nearly five hundred feet more above tide-water. The elevation above the level of the ocean does not, therefore, in all probability, much exceed eleven hundred feet. And this is about the height ascribed to Anthony's Nose, in the Highlands of New-York, by Mr. Knight.

The elevation is, nevertheless, considerable enough to influence its temperature. The heats of summer are not so great as in the valleys. Droughts are less common and pinching. Snow falls earlier, and lies longer than in the adjacent plains. The warmth of a copious spring of pure water, as it issued out of the sand near the top of the mountain, was only 50 degrees, while the temperature of the water gushing from the briskest springs on the north side of Long-island, and drawn from the deepest wells at New-York, is 54 degrees. The spring water on the summit of Schooley's mountain is, therefore, four degrees colder than that around New-York.

This mountain is not a mass of stratified rocks, piled upon each other from bottom to top. There is no peculiar difficulty in travelling over it. The predominating materials are clay and sand, forming a good loam; which, though generally not argillaceous enough for the formation of bricks, is, at the same time, gravelly enough for the growth of grass and grain. Yet rocks are thickly distributed over its face and along its sides. They are mostly detached, though some of them are of large dimensions. They consist chiefly of feldspar and quartz: the quartz is prone to be semipellucid, and is granular or angular, resembling

coarse marine salt. The feldspar is mostly whitish, sometimes reddish, and presents less of the polished fracture than the American feldspars usually do. It has the appearance of a more imperfect formation, or of having undergone a partial decomposition. These two ingredients make up the bulk of the rocks. Many masses may be examined without observing a vestige of mica. Abundant as mica is almost every where in these parts, with the mixtures of feldspar and quartz, in our primitive rocks, it is remarkably deficient here. Now and then a little schistus, or horneblende, is found embodied and compacted with the quartz and feldspar. Grains of yellow pyrites also sometimes occur. Rust, ochre, and other indications of iron, are dispersed extensively both through the rocks and the soil. Iron ore is indeed so plentiful, that furnaces are in operation both in the eastern and western districts of the chain. Much of it is magnetical, and its action is so powerful upon the needle, that surveyors of land often find it very difficult to employ the compass. It would be possible to collect great quantities of the magnet, and of other ores of iron in the middle region. Towards the foot of the hills, limestone is found skirting the valleys along, and is calcined in quantity sufficient for all œconomical uses.

Among the natural productions thereabout, are masses of excellent flint stones. They lie along the valleys and side hills, where they have been washed bare; and are sufficient in quantity and quality for domestic supply of our musketry. They are more pure and of a better fracture than those contained in the lime-stone near Niagara. And when this important article of public defence shall be thought worthy of being improved by the citizens, there seems to be in New-Jersey an inexhaustible supply for our fire-arms.

A turnpike road has been completed from the city of Jersey, at Powleshook, to the summit of Schooley's mountain. The travelling is excellent the whole distance. This is just fifty miles from New-York city. Estimating the width of the Hudson to be two miles, the distance to Newark is nine, to Springfield seven, to Chatham five, to Morristown seven, to Mendham six, to Blackriver six, to Dutch-valley five, and to the Mineral Spring on the eastern or further side of the mountain, three miles. Through such a succession of thriving villages, and amidst a country pleasingly checkered with forests and farms, the rise of the first five hundred feet is surmounted in about forty-seven miles, as the traveller passes over a surface of easy elevations

tions and depressions. The remaining six hundred feet are ascended in less than two of the remaining miles, between Dutch-valley and the summit. The principal part of the remainder is a descent to the Spring on the opposite declivity.

An able horse will carry a chair hither from New-York in a summer's day, or return thence between the rising and setting of the sun. From the top of the mountain one finished turnpike is continued northward, to Sussex, another westward, to Eastown, and a third eastward, to New-York. It is in contemplation to open a fourth from the same point, to proceed in a course southwardly direct to Trenton.

The Mineral Spring which has been mentioned has given much celebrity to the neighbouring region. It is said to have been known to the native Indians, and to have been employed by them as a remedy. The white people have resorted to it almost ever since the settlement of the country. Remarkable cures are ascribed to it: and some persons have been in the habit of visiting it season after season, for the purpose of being benefited by its wholesome properties.

It is situated in the town of Washington, in the county of Morris. It is, in strictness, a rill which issues from a fissure in the perpendicular side of one of the above-described rocks, on its eastern exposure. The place of discharge is, perhaps, between forty and fifty feet above the level of a brook which gurgles over the stones, and foams adown the rocks in its channel beneath. The extremity of a wooden leader is so adapted to the crack in the rock as to receive the water, and convey it to the platform where the drinkers assemble, and to the recesses whither the bathers retire.

Its temperature is rather more than six degrees warmer than that of the spring water near the summit. The mineral water, as it pours from the spout, possesses a heat somewhat warmer than fifty-six degrees. This is about the same which the slower springs and the shallower wells around New-York possess.

The quantity of water which it affords can easily be measured. By experiment, it appeared to discharge a gallon in about two minutes and a half. At this rate, the amount would be twenty-four gallons per hour. But allowance is to be made for leakage and waste, inasmuch as the conduit does not collect the whole. Suppose this to be six gallons more. Then the quantity running out will amount to thirty gallons per hour. Some trials are reported to have

shown a rather more abundant flow. On the whole, it may be stated with tolerable correctness that the fountain within the bowels of the mountain emits, from this opening, a quantity of water not varying greatly from a barrel per hour, or six hogsheads per day. The quantity is not observed to vary under any changes of season or weather.

The spouts which convey the water are lined with a yellowish deposit. The like sediment incrusts the reservoirs at the bathing-house. The earth and stones through which the water soaks away, present a similar ochreous appearance. Where the boards contain astringent matter, a dark purple or blackish colour is formed.

The presence of iron being thus indicated, a few experiments were made to determine the matter more clearly.

A bright blue was produced on adding the prussiate of potash to the water.

Green leaves of the common chesnut-tree, (*fagus castanea*,) on being bruised and infused in the water, formed a pale purple.

Those of chesnut-oak (*quercus prinus monticola*) yielded a brighter purple.

Those of the sumach (*rhus glabrum*) quickly turned to a purple.

Fresh lacerated leaves of the maple (*acer rubrum*) immediately formed a deep purple.

Hickory leaves (*juglans vulgaris*) made a faint dusky hue.

Black-oak leaves (*quercus nigra*) struck a darker colour.

Butternut leaves (*juglans cinerea*) afforded a dusky brown.

The waters of the spring, mixed with brandy, made a mixture of a dark and unsightly colour.

An infusion of green tea formed browns, purples, and blacks, according to its strength and proportion.

The chalybeate character of the water being thus established by so many tests, attempts were made to ascertain whether there was any gaseous impregnation.

For this purpose glasses were inverted in a convenient vessel, receiving the stream immediately from the spout. But not a bubble of air was collected, other than common spring water affords.

To determine whether any carbonic acid was combined with the water in a form not spontaneously separable, lime-water was mixed with it; but no change of colour was perceptible in the mixture.

Various proportions of the spring-water and lime-water were

were mingled in repeated experiments, without effecting any cloudiness or causing any precipitate.

Afterwards, as a test to the goodness of the lime-water, the milky hue and carbonic precipitate of the lime was instantly produced, by breathing through a tube into the mixture of waters, air which had undergone the respiratory operation of the lungs.

There was thus no evidence of any carbonic acid at all.

When the water of the spring was suffered to stand in the open atmosphere, and acquire the summer temperature, by receiving twenty or more degrees of heat, some air bubbles were distinguishable on the sides of the vessel; but they were only such as any cold water would exhibit under equal circumstances.

As there was no calcareous incrustation at the spring, there was reason to believe the water destitute of lime. On adding to it oxalic acid, there was no change of colour produced. Whence it may be inferred that lime makes no part of the constitution of this fluid.

To enable a judgement to be formed whether any other earths were combined with the water, soda and potash were severally and repeatedly added. The precipitates were, however, so small, and so slowly produced, that there was ground to suppose the presence of earthy matter was very inconsiderable, and that there was no metallic impregnation except that of iron.

The nitrate of silver caused a whitish appearance; but not in so considerable a degree as it does in the water of New-York, constantly drunk by the inhabitants. The cloudiness was indeed not more considerable than rain water along the sea-coast is occasionally known to present when subjected to the same test. The tinge of muriatic acid hereby indicated, probably arises from a faint solution of sea salt.

There is notwithstanding a weak acid of some kind in the Schooley's mountain mineral water. If litmus paper be exposed to the water as it issues from the rock, the blue is gradually changed to a reddish; and on the addition of an alkali, the acquired colour vanishes. What the nature of this inconsiderable portion of uncombined acid may be, is not perfectly easy to determine. Its presence is attended with the flavour which water derives from running over decayed leaves, and draining through a soil abounding with the living and dead roots of trees, shrubs, and sylvatic plants. Former experiments have proved to me the existence of an acid in the rotten wood which overspreads the American forests.

forests. And water passing through a stratum of vegetable mould is known to receive what is called the *woody* taste. It is therefore presumable that the rain water receives a tincture from the thick layer of vegetable mould through which it is strained, and carries the flavour of it to the fountain. The peculiarities of this feeble acid, like that of numerous others we meet with in practice, does not seem to be defined in chemistry as yet by discriminating characters.

The iron of this mineral water is very easily separated. Exposure to the atmosphere is followed by a metallic precipitation. Transportation to a distance, as bottles are commonly corked, is attended with a deposition of the iron. The water, after having been carried to New York, when subjected to experiment in my house, gave no evidence of a chalybeate quality when tested by the Prussian alkali and spirituous tincture of galls. This same water, after being boiled in a kettle, makes excellent tea. The heat of ebullition seems to separate the ferruginous ingredient, and the infusion is thereby freed from all dusky or black tint. Still, if this same infusion of green tea is mixed with water fresh from the spring, a dark and disagreeable hue is instantly produced. A short exposure to the heat of 212° thus converts this mineral water into a good tea-water. It is employed for this purpose occasionally.

If there is any thing that deserves the name of a pure chalybeate water in the world, this would seem to be such a composition. The iron appears to be united with the water without the aid of carbonic, or indeed any other acid; for the weak acidity detected by litmus can scarcely be considered as contributing to its solution. Some part of the iron ore universally diffused among the minerals hereabout, is in a state proper for water to act upon, and to produce the martial impregnation remarkably free from other admixtures.

Schooley's Mountain, July 10, 1810.

XXXVII. *Case of Hernia successfully treated by an Operation.* *By* JOHN TAUNTON, *Esq. Surgeon to the City and Finsbury Dispensaries, to the City of London Truss Society, Lecturer on Anatomy, &c. &c.*

NOV. 22, 1810.—MARY MANNING, æt. 67, of a good constitution, although she has lived rather freely, much addicted

dicted to the use of spirits, and has been at times deranged. She has been subject to hernia in the left thigh for many years, but it never attained any considerable size; nor has it been much noticed, and no truss was ever applied.

On Friday last, the 16th instant, she was seized with pain over the abdomen, which extended to the lumbar region, but was most severe at the umbilicus and in the scrobiculus cordis, attended with a sensation of heat.

On Saturday the 17th the abdomen was more tense—great perspiration, hiccough, sickness and vomiting. On the 18th these symptoms were more violent; and on the 19th feculent matter was vomited. Dr. Hancock, who was called in, ordered fomentations to the abdomen, and aperient medicines. On the 20th the symptoms were still increasing, and the oleum ricini was ordered.

On the 21st symptoms increased: calomel and scammony were administered, but every thing was rejected by the stomach. The symptoms at length induced Dr. H. to suspect hernia; and on the 22d I was requested to visit the patient, which I did at 1 P. M.*

It appeared to me that no time ought to be lost in performing an operation, as the hiccough and vomiting of feculent matter were very distressing, and the countenance indicated great constitutional irritation, although the pulse was moderately firm and not very quick. The tumour was very small, being scarcely perceptible except when the patient was lying on her back. The operation was acceded to and performed at 3 P. M.

The hernia was situated high up, nearer to the ilium than to the pubis, and was very firm. On dividing the integuments, the adipose cellular substance and fascia propria, the hernial sac was brought into view; it was extremely thin; and on dividing it a portion of firm omentum appeared, somewhat discoloured: on the inside of this omentum there was a fold of one of the small intestines greatly discoloured and thickened, so deep seated as not to form any part of the tumour seen externally: some fluid was also contained in this part of the sac, but not any above where the omentum was situated. The stricture was so deep as to take the whole length of the bistoury, and it was dilated inwards and downwards towards the pubis. The intestines and omentum were returned; and the wound was secured by two sutures, straps of adhesive plaster and lint.

* The patient never ascribed her illness to any thing like hernia, and submitted to an examination of the parts with great reluctance, on the subject being suggested to her.

On visiting the patient in the evening, she had had two evacuations by stool, and the sickness and vomiting had entirely ceased.

℞ Liquor ammon. acetat. cum aq. menth. et sp. lavand. comp.

23d.—Has had several stools during the night, of a watery consistence, but has had some sleep and no return of bad symptoms.

24th and 25th.—Much better, bowels regular.

26th.—Some discharge from the wound, but she appears quite well; has been twice out of bed, and has a good appetite.

27th and 29th.—Dressed the wound, on which there is some erysipelatous inflammation which extends towards the pubis; some pain in voiding urine.

℞ Liquor plumbi acetat. dilut. Fiat lotio.

A few days afterwards a considerable degree of pain came on in the abdomen, particularly on the left side, attended with hiccough and sickness, but the bowels were regular. It appeared, however, that this inflammation was brought on by the patient having returned to the use of spirits.

℞ Liquor ammon. acetat. ipec. comp. syrup. papav. —ter die.

About the middle of December the wound was perfectly healed, and the patient was dismissed cured.

JOHN TAUNTON.

XXXVIII.—*On the Action of the Vegetable Acids on Alcohol, with or without the Intermedium of the Mineral Acids.—By* M. THENARD *.

SCHEELE is the only chemist who has hitherto taken up the subject of this memoir. At first he was convinced that the acetic, benzoic, tartarous, citric, and succinic acids did not form any ether with pure alcohol. Afterwards, being desirous of ascertaining if these acids were susceptible of forming it with alcohol and a mineral acid, such as the sulphuric, nitric, and muriatic acids, he made various experiments; from which he concluded:

1st. That the acetic acid and alcohol produced, with one of the three mineral acids above mentioned, an ether which was easily decomposed by potash, and which contained acetic acid.

* *Mem. d'Arcueil,* tome ii. p. 5.

2d. That

2d. That the benzoic acid and alcohol formed with the muriatic acid a kind of oil heavier than water, which we might decompose by potash like acetic ether, and of which one of the constituent principles was benzoic acid.

3d. That we obtained no particular product by treating an alcoholic solution of tartarous, citric, or succinic acid, either by sulphuric, nitric, or muriatic acid.

Scheele knew, therefore, that acetic ether and *benzoic oil* contained, the one acetic acid and the other benzoic acid; but he was not acquainted with the other principles which enter into them, nor, *a fortiori*, with the part acted by alcohol and the mineral acids in the formation of these two kinds of compounds. From this alone we may suppose that, if he had inferred from his experiments some consequence relative to the formation of these compounds, he would have admitted into the acetic ether, and into the benzoic oil, a vegetable acid, and, in addition, the mineral acid which he employed and some alcohol, or rather merely a new body proceeding from the alcohol decomposed by this mineral acid; or perhaps this new body and this mineral acid itself.

This triple hypothesis shows how incomplete the experiments were upon which it was founded. It was therefore necessary to resume the subject, and I have done this with the more care because it is immediately connected with my own experiments on the ethers, and because the results to which they led me are very singular, and may even become important.

My operations, like those of Scheele, are divided into two parts: *i. e.* I examined in succession the action of the pure vegetable acids, and of the vegetable acids mixed with the mineral acids, on highly rectified alcohol.

Almost all the vegetable acids are dissolved in alcohol, and separate from it on distillation without any particular products resulting, however frequently we distil the same portion of alcohol with the same portion of acid;—such are the tartarous, citric, malic, benzoic, oxalic, and gallic acids: and I have no doubt, although I have not made the experiment, that the suberic, succinic, mucous, pyro-tartarous, moric, and honigstic acids are in the same predicament. But this is not the case with the acetic acid: its reaction on alcohol is such, that by means of several distillations, the two bodies disappear and form a true ether: hence I conclude that this acid is probably the only one, of all the vegetable acids at present known, which exhibits the above or any other analogous phænomenon.

But when, instead of placing the vegetable acids in contact

tact with alcohol, we put them at the same time in contact with that body and one of the highly concentrated mineral acids, we may then produce with the whole new combinations very remarkable in their nature, as I shall prove by the following experiments:

Experiment I.—I took 30 grammes of benzoic acid, which I dissolved in 60 grammes of alcohol; I introduced the solution into a tubulated retort, and I added 15 grammes of concentrated muriatic acid; afterwards, having adapted a tubulated bell-glass to the neck of the retort and a bent tube to the bell-glass, I proceeded to distillation, and I ceased when it was two-thirds finished.

Throughout the whole course of the operation, nothing but atmospheric air was extricated, and scarcely any traces of muriatic ether. The first portions of the product distilled were merely alcohol; but the last portions contained a peculiar substance, which could be separated from it by means of water: there was a good deal of this matter at the bottom of the retort, where it was deposited upon cooling; and as it was covered by a mixture of alcohol, water, muriatic acid, and benzoic acid, I purified it by decantation and by washing it with warm water, in which but very little of it was dissolved. When thus purified, it was yellowish, a little heavier than water, pungent, fusible at the temperature of 25° or 30°, volatile at about 80°, acid, oleaginous, almost insoluble in cold water, less insoluble in boiling water, from which it was precipitated upon cooling; and very soluble in alcohol, from which we might precipitate it by water. It evidently contained the benzoic acid, which gave it the property of reddening turnsole. When brought to the neutral state by an alkaline solution, it was white, always pungent, and always odoriferous; it constantly possessed most of the foregoing properties, and besides it was perfectly liquid at the ordinary temperature: finally, when shaken for a long time with a solution of caustic potash, it disappeared without any gas being extricated; and when this solution was examined, no trace of muriatic acid was found in it, and, in fact, nothing was procured from it but benzoic acid and alcohol. This substance, therefore, which exhibited itself as an oleaginous one, and in which there was apparently no acid, is formed of alcohol and benzoic acid in a particular state of combination: nevertheless we could not obtain either, by distilling alcohol and benzoic acid together a great number of times, or by precipitating by means of water a solution of benzoic acid in alcohol, or by strongly concentrating this solution

and

and leaving it to itself. Thus, although the muriatic acid does not form part of this singular substance, and although the two bodies which form it are present, it cannot be produced except with the concurrence of this acid; a consequence which, extraordinary as it may appear, is nevertheless perfectly accurate, and which we shall presently account for: in the mean time let us see if the other vegetable acids would not be in the situation of the benzoic acid with respect to the alcohol.

Experiment II.—When we make a solution of 30 grammes of oxalic acid in 36 grammes of pure alcohol, and, after adding 10 grammes of concentrated sulphuric acid, distil it until a little sulphuric ether begins to be formed, nothing but alcohol slightly etherized passes into the receiver, and there remains in the retort a brown liquor very strongly acid, from which, upon cooling, nothing but crystals of oxalic acid is deposited: but when we dilute this liquor with water, there is separated from it an abundance of matter similar to that which the benzoic acid has given to us, imperfectly soluble in water, and which may be obtained pure on washing it in cold water, and by taking from it by a little alkali the excess of acid which it retains.

If we treat the citric and malic acids in the same way, we obtain absolutely the same results. The three substances proceeding from these three acids resemble each other in some of their properties; all of them are a little yellowish, a little heavier than water, without smell, evidently soluble in alcohol, from which they are precipitated by water. They differ from each other in point of taste: that which is made with oxalic acid is feebly astringent; that which is made with citric acid is very bitter. I am not acquainted with the taste of the other. The first is the only one which is volatile; it is more so than water, and by this means we easily obtain it clear. It would be very interesting to know the nature of the whole: I was naturally led to suppose, that by distilling them with a solution of caustic potash I should decompose them, and that the first would give me oxalic acid, the second citric acid, the third malic acid; that the whole would afford alcohol, and that none of them contained sulphuric acid:—all this in fact took place. Here, therefore, we have new combinations of vegetable acids and of alcohol, in the formation of which the sulphuric acid acts in the same manner with the muriatic acid in the foregoing experiment.

It became highly probable from the above, that all the vegetable acids would act without alcohol in the same way as the

the foregoing acids. In order to prove it, however, I was desirous still to subject some to a rigorous test, and with this view I selected the gallic, tartarous and acetic acids, the other acids being difficult to procure, or being insoluble in alcohol.

With the gallic acid, the experiment had not all the success which I desired, because I operated upon 10 grammes of acid only: the combination constantly took place; for, after having distilled nearly one half of these 10 grammes of gallic acid with 12 grammes of alcohol and four grammes of sulphuric acid, I found in the retort a liquor which, being diluted with water and saturated by potash, yielded on a new distillation all the free alcohol which it contained, and which, when mixed with an excess of potash, furnished me still another portion, which certainly must have been combined with gallic acid.

With the tartarous acid, the experiment on the contrary succeeded completely, and exhibited curious results. Here, as in the case of the oxalic acid, I employed 30 grammes of vegetable acid, 35 grammes of pure alcohol, and 10 grammes of concentrated sulphuric acid. I distilled until a little ether began to be formed: at this period I put out the fire and allowed the retort to cool slowly. Upon cooling, the liquor became a thick syrup. I poured water into it in vain, in the hope of separating, as in the preceding experiments, a particular combination of acid and alcohol: then, having added successively different quantities of potash, I precipitated from the liquor plenty of acidulated tartrite of potash; afterwards, having saturated it without exceeding the point of saturation, having evaporated it, and treated it in the cold way by highly concentrated alcohol, I obtained by the evaporation of the alcoholic solution a substance which, upon cooling, became a thick syrup, more easily still than before having been treated by potash and alcohol.

This substance has a brown colour, and is slightly acid, nauseous, inodorous, by no means acid, very soluble in water and in alcohol: it does not precipitate the muriate of lime; it precipitates abundantly the muriate of barytes; when we calcine it, it gives out thick vapours which smell of garlic, and at the same time it leaves a charry residue, not alkaline, which contains much sulphate of potash:—in a word, when we distil it with potash, we extract from it some very strong alcohol and a considerable quantity of tartrite of potash. It is evident, therefore, that this substance is also a combination analogous to the foregoing; but

but what is very remarkable is its syrupy state, and the property which it has of rendering very soluble in highly concentrated alcohol the sulphate of potash, which by itself is insoluble in weak alcohol. Perhaps it is to the sulphate of potash that it owes the property which it has of not having the oleaginous appearance of all the other combinations of this kind.

The experiments on the benzoic, oxalic, malic, citric, gallic and tartarous acids being finished—those on the acetic acid remain to be detailed. I depended the more on these experiments, because by varying them they throw much light on the true manner in which the mineral acids act in the formation of the new combinations under consideration. In all these experiments I made use of alcohol at 800 of specific gravity (temperature 10° centigrade), and of acetic acid capable of crystallizing at 0.

Experiment I.—I distilled once only a mixture of 30 grammes of alcohol and 20 grammes of acetic acid: it required a great deal of heat to boil the liquor, and with great difficulty a few grammes of acetic ether were formed.

Experiment II.—I repeated the above experiment, adding to the mixture of alcohol and acetic acid five grammes of concentrated sulphuric acid: 19 grammes of acetic acid disappeared; the ether was formed with singular facility and almost without heat. I obtained 40 grammes of it. It follows that this process is excellent for making acetic ether, and far superior to that in common use; on the one hand because we obtain much more ether, and on the other because there is no occasion for several distillations. Besides all this, the rectification of this ether is very easy: it is only necessary to add a little potash and to decant; for the acetate of potash which is formed is collected at the bottom of the vessel.

We may also make an excellent ether in an œconomical way, by taking three parts of acetate of potash, three parts of highly concentrated alcohol, and two parts of sulphuric acid, also highly concentrated: introduce them into a tubulated retort, and distil the mixture to perfect dryness; mix the produce with the fifth part of its weight of sulphuric acid also highly concentrated; and after a careful distillation, as much ether will be procured as there was alcohol employed. We may substitute any other acetate for the acetate of potash, and particularly the acetate of lead; but in this case we must employ other proportions of alcohol and of sulphuric acid than those above mentioned.

Experiment

Experiment III. — When we employ less than five grammes of concentrated sulphuric acid, in order to convert 20 grammes of acetic acid into ether, the experiment has but a partial success.

The success of the experiment is partial also when the sulphuric acid is diluted with water: it would be completely unsuccessful if the acid contained much water.

Experiment IV.— When we use concentrated nitric or muriatic acid for the conversion of acetic acid into acetic ether, more of them is requisite than of sulphuric acid, and still more in proportion as they are diluted with water.

Experiment V.—The phosphorous acid, brought to a syrupy consistency, also facilitates very much the formation of the acetic ether; but the quantity of this acid must be equal at least to two-thirds of the quantity of acetic acid, in order that the whole of the latter may disappear upon the first distillation.

Experiment VI.—The arsenic and oxalic acids favour the formation of the acetic ether in a very slight degree.

Experiment VII —The tartarous acid does not favour it.

Experiment VIII.—The sulphurous acid gas does not favour it, although it is extremely soluble in alcohol, and although it produces much heat on being dissolved in it.

Experiment IX.—It is the same case with the phosphoric acid; but it is because this acid is insoluble, or scarcely soluble, in alcohol.

If we examine the result of these experiments, we see that all the acids which can condense alcohol favour the formation of the acetic ether, and that they favour it the more, the more strongly they condense alcohol For this reason, the sulphuric acid favours it most, and the tartarous acid does not favour it sensibly; and when the sulphuric acid itself is too much diluted with water, it is in the situation of the tartarous acid. We must therefore suppose that, when the alcohol has been thus condensed by an acid, the acetic acid seizes it, and constitutes the acetic ether, combining in a particular manner with it. Now we cannot but admit a similarity of action in the strongly concentrated acids, either in producing the acetic ether or in combining the other vegetable acids with alcohol: consequently in the whole series of combinations which we have observed, and which would not have taken place without a powerful mineral acid, this mineral acid only acts by condensing the alcohol, and by bringing it to the state at which this body may be united with the vegetable acid.

We

We may therefore establish the following principle, which expresses in a general manner what I have detailed in this memoir.

When the vegetable acids are pure, there are none of them, if we except the acetic acid, which can, by combining in any manner with alcohol, lose their acid properties; but when they contain a mineral acid capable of strongly condensing the alcohol, all these acids form, on the contrary, with this body, a combination of such a nature that their acid properties disappear, and yet without the mineral acid making part of the combination.

This principle being recognised, there is no reason why it ought not to include the animal acids. Probably it will extend to the mineral acids, and we shall thereby discover the means of combining them easily with alcohol; and even perhaps we shall be permitted to combine all the vegetable and animal substances, if not with all the acids, at least with those which are strong and concentrated. What is at least certain is, that it may become fertile in results, since it increases our means of combining matter.

However the case may be, I shall proceed with my inquiries. I shall inquire whether the mild oil of wine is not formed of alcohol and sulphurous acid; and if the kind of oil which we obtain on passing oxymuriatic acid through alcohol is not itself composed of alcohol and muriatic acid. I shall examine the properties of the various combinations which I have made known. I shall try to decompose them by different salts, and thus to combine, by means of double decompositions, alcohol with all the mineral acids. I shall above all endeavour to ascertain if there exists a perfect identity between those kinds of combinations the formation of which is indirect, and the nitric and muriatic ethers the formation of which is direct. Finally, I shall try to determine if really (condensation excepted) the mode of combination is the same when a vegetable acid is dissolved in alcohol without losing its acid properties, and when on the contrary, by being intimately combined with this body, its acid properties disappear.

XXXIX. *Extract from a Memoir on the Topography and the upper Strata* (le Relief) *of the Soil of Paris.* *By* P. S. Girard *.

The plains on which the city of Paris is situated were formerly covered by the waters of the Seine, whenever that river

* *Nouveau Bulletin des Sciences, par la Société Philomathique.* Paris, 1808.

river was swollen beyond its ordinary dimensions. The gravel which it carried with its stream and the mud which it kept suspended after great rains were deposited on the surface of the meadows, and every year a new layer of these deposits raised the soil, while at the same time repositations of a similar nature raised the bed of the river: thus the bottom of the river and the soil of the meadows would have continued to rise nearly simultaneously, by the deposit of the substances which the Seine brought from the higher grounds, if the necessity of warding off the effects of inundation had not compelled the first inhabitants of the small island of Lutece, and those who subsequently came to inhabit the two opposite banks of the Seine, to accelerate the work of nature by bringing new accessions of soil to the grounds which they occupied, by raising on the banks of the river either dykes or quays to defend them from its encroachments.

It was therefore at the earliest period in the history of Paris that the soil on which it was situated began to be raised artificially, and since this period the bed of the Seine has been slowly rising; but from the natural effect of a cause always acting, which raised at the same time the level of the inundations, it became necessary, to provide against the latter, to raise the level of the quays and grounds adjoining by adding loads of earth in proportion.

As this labour is confined to heaping up rubbish on one point of the valley rather than on another, it seems foreign to the subject of geology: besides, the small number of facts which history has transmited as to the changes which have brought the materials of the soil of Paris to their present state, have been collected accidentally only, and are connected in some measure with circumstances of another nature.

While Paris was a fortified city, surrounded by ditches and walls, the materials which proceeded from the daily demolition of the ancient edifices, which were replaced by new buildings, could not be permitted to remain within the walls: the narrowness of the streets and the height of the houses, in the more ancient parts of the town, sufficiently prove that the ground was too precious to dedicate any part of it to the storing up of rubbish, which was taken outside of the walls, and deposits of it formed near to the principal gates.

In this manner were formed outside of the two great circumvallations of Paris, made in the reign of Philip the August and Charles IX., the mound Saint Roch, or des Moulins, between the ancient gates of St. Honoré and Mont Martre; the

the mound of *Nôtre Dame de bonne Nouvelle*, between the ancient gates of Montmartre and St. Denis; the mound De la Rue Meslay, between the gates of St. Martin and of the Temple.

And upon the opposite banks of the Seine, the mound De la Rue St. Hyacinth, between the gates of St. Michael and St. Jaques, the mound at the extremity of Rue Taranne; and finally, that which is at present the Labyrinth of the Jardin des Plantes. The last two have been formed of the rubbish which came out of Paris by the gates Bussy and Saint Bernard, and from that which came from the Fauxbourgs St. Germain-des-Prés and St. Victor.

The various mounds now described form the only remarkable protuberances on the present surface of Paris. The diggings which are daily made into them prove that they are composed of adventitious materials; and if this fact were not in a manner proved every instant, it would be difficult to account in any other way for the formation of such mounds in the midst of a level exposed to periodical inundations.

When these mounds had acquired a certain elevation above the adjoining edifices, windmills were erected on their summits. The old plans of Paris point out these edifices, which existed even some time after the eminences in question had been included within the last inclosure made of Paris, about the year 1634. The population still increasing, these mounds were levelled, and new houses built upon the spot.

The fortifications of the capital having been demolished and its ancient ramparts converted into a promenade, there was nothing to hinder the suburbs from extending. It was at the commencement of the last century that the Fauxbourg St. Germain rose on the left bank of the Seine on the Pré-aux-Clercs. The Fauxbourgs situated on the right bank of this river between the Boulevards and the hillocks of Montmartre and Roule have an origin still more recent. The ground which they occupy has been raised by rubbish, at first forming various causeways more or less elevated above the meadows, and following the alignement of the new streets. The spaces between these causeways have been successively raised to their level.

It is upon these artificial soils that the quarters of Paris are built which project in the marshes of the Temple and of Popincourt, and at the foot of the mound Roule. We there see new causeways passing through garden grounds, until these very grounds are in their turn covered with rubbish on which new buildings will be erected.

After having ascertained the various causes which have

concurred to raise the soil of Paris and to give the appearance to its surface which it now has, it may be necessary to describe this appearance with the greatest possible accuracy.

The simplest and most expeditious mode of judging, in order to obtain certain results capable of being graphically represented, has been to indicate on a plan of Paris the height of the different points of its surface above or below a determinate horizontal plane.

The scale of the plan drawn by Verniquet was large enough to admit of the smallest differences in point of height being rendered perceptible. This great topographical work, therefore, presented us with the means of facilitating the task which we undertook.

We began with levelling the right bank of the Seine: we divided, by great lines directed from east to west and from north to south, the whole space contained between the river and the new Boulevards: we thus obtained profiles which were connected with each other and with a common index. When we were well assured by the necessary verifications of the accuracy of these profiles, we divided by new lines the great spaces contained between the directions of the former, and we obtained still closer outlines. All these having been verified in their turn, we divided by more multiplied lines the space comprehended between the second lines of operation, and so on, multiplying the strokes of the level more and more until we obtained the line of intersection of all the streets of these quarters. In addition to all this, we chose a great number of points, the heights of which were laid down in a separate sheet, and which were distinguished on Verniquet's plan by a particular notation; precautions by means of which these points will serve not only to verify the heights already found, but to multiply their number if that were necessary.

The heights of the soil at the intersections of all the streets being determined, we searched between two consecutive intersections, and supposing the slope of the ground to be uniform, for one or more points which were at heights determined with respect to the plan of the level, which is supposed to be raised 50 feet above the surface of the water of the basin of la Villette, or, what is the same thing, about 26 metres above the Seine at low water.

We afterwards joined by straight lines all the points which were found at the same height. This gave the trace of a polygon, which evidently represents the intersection of the surface of Paris by a horizontal plane.

We repeated this operation for every metre in height, and

and obtained a series of irregular polygons, the trace of which indicates the successive intersections of the surface of the soil by horizontal planes raised one metre above each other.

These polygons, more or less distant from each other as the slope of the ground is more or less rapid, indicate to the eye in the most palpable and rigorous manner, the inclination of the hillocks which run parallel with the banks of the Seine, as well as the factitious mounds which we remark in the bottom of the valley, and which we have already mentioned.

The general survey of the city of Paris was finished in two years. It required operations in detail of a most multifarious kind; and I have no doubt that, by following the order and course which I have pointed out in another memoir *, we shall in a few years be in possession of results equally satisfactory with respect to the general survey of France.

XL.—*Observations and Experiments concerning Mr. Davy's Hypothesis of Electro-chemical Affinity.*—*By* M. Donovan, *Esq.*

Mr. Davy's hypothesis of the identity of chemical and electric attraction, although ably supported by a variety of strong facts, ingenious reasonings, and happy illustrations, is not confirmed by the concordance of known phænomena. Between these powers so many differences are observable, the one is so often produced where the other can scarcely be supposed to operate, or, according to what is known, ought to operate differently, that we cannot with any degree of confidence rest on the assumption of their similarity; the less so, when it is reflected that the whole is supported by probabilities only, without even the profession of a single proof. However truly philosophical may be the design of referring all the operating energies of nature to one great universal cause; yet must we proceed with caution, and not be misled to pronounce causes identical before we are convinced of their real connexion. The limited perceptions of man do not allow him to survey the chain of causes at one steady comprehensive view. There must at length be a break; and when he loses sight of the connexion, he can no longer pronounce upon identity.

The reasonings in the following pages rest not on a hy-

* *Journal des Mines*, tom. xvii. p. 297.

 pothetical

pothetical assumption of two electric fluids, of one, or of none. In pursuing every investigation, we must reason on doubtful points by the assistance of those that are certainly known on the subject. That of this power, whatever be its nature, there exist two modifications, each of which has a strong tendency to approach the other to an insensible distance, is proved beyond the possibility of doubt; proved by the only means of decision in any matter; namely, the evidence of the senses. It is proved by no less a certainty, that similar modifications have a tendency to recede from each other; and that where the dissimilar powers are at liberty to come into contact, they effect a mutual change which deprives each of its relative properties. These are all the postulates necessary to the following reasonings; and it is of no consequence what expressions are used, nor whether positive electricity be considered as plus, and negative as minus; nor whether both are merely properties of matter, and not the agency of any fluid *sui generis*. The facts are indisputable, notwithstanding the insufficiency of language. But as the language of the hypothesis which supposes the existence of two fluids is most easy of expression, it shall be hereafter adopted.

Mr. Davy has stated his opinion, that the effects attributed to a certain force called affinity, exerting its energy between the ultimate particles of heterogeneous matter, may be no more than the operations of electricity; and thinks that all the phænomena of affinity ought to happen conformably to the known principles of electricity.

He supposes that combined bodies are in differently electrical states: that acids and oxygen are in the negative state; that earths, alkalies, metals, oxides and inflammables are in the positive state: that the attraction of these different powers is the force which sustains these substances in combination.

He illustrates such a combination by the example of Beccaria's glass plates, differently electrified, which cohere with great force, and adds, that "different particles in combining must be still supposed to preserve their peculiar states of energy."

He instances a variety of substances, as metals, acids and alkalies, which by a few contacts become electrical in different states.

Mr. Davy lastly supposes that artificial electricity effects the decomposition of those compounds, by attracting the different and by repelling the similar power.

Mr. Davy, it is plain, sets out with two assumptions.

1st. That

1st. That bodies which possess for each other a chemical affinity are in different states of electricity.

2d. That these bodies after combination still retain their different electricities.

These Mr. Davy has not proved. Were they allowed, he certainly succeeds in establishing some of his points. But are we to grant for that reason what is entirely in opposition to the laws of electricity, which have been founded on the firm basis of experiment?

In opposition to these two assumptions, I shall endeavour to show:

1st. That permanently supernatural states of electricity cannot exist.

2d. That different states, after union, no longer display sensible properties.

Entering on the inquiry, the question naturally occurs, How do these substances, when about to combine, acquire differently electrical states? Whether do bodies possess an independent state of power, absolutely existing; or is it always acquired in consequence of separation after contact? The former can scarcely be allowed, from the following considerations:

1st. Electricity can only exist in three states: in the naural, in the positive, or in the negative state.

2d. The first is the state in which electricity always insensibly exists, until some action be exerted on it which causes it to become sensible, and then it appears in either the positive or negative state.

3d. The positive and negative are forced states, which can only exist while the primary exciting cause continues, unless they be detained by substances which allow of but a very slow passage. Even then, so transitory is their existence, that they will insensibly disappear, once more forming natural electricity.

These are facts that cannot be controverted. Sulphur, wax, glass, resin, silk, &c., when excited either strongly or feebly, will always in a little time return to their natural state. The stronger electricities of charged conductors and the condensed powers of batteries soon undergo the same change. Thus we are acquainted with no substance which has the property of retaining a forced state permanently.

From observation of the fact, that some bodies when brought into contact and afterwards separated show signs of being differently electrified, it might appear that the attraction between such bodies would be a sufficient cause of

 combination.

combination. For at every effort towards separation the attraction begins to act, which apparently would permanently unite them.

Mr. Davy has no where expressly mentioned his opinion that electric energies may be produced in this manner. Yet that he has it in view, I think, is deducible from his illustrations of the electricities evolved by separation after contact, and of the energies being often increased by heating; all of which are apparent after separation only.

The improbability of this opinion becomes apparent, when it is considered, that although bodies are rendered electrical by separation, yet there are no grounds for supposing electricity evolved by contact. On the contrary, it is certain that forced states cannot exist together for any considerable length of time, especially if the contiguous bodies be metallic: and accordingly such bodies, while the contact continues, display no powers. Even by the hypothesis, electricity is rendered sensible by separation only. Now as it will readily be granted that a power cannot act before it is created, I would ask, What is the cause of the combination of solids, which are attended by a complete change of properties, by a violent evolution of heat and light, and other marked effects; which solids, from their state of existence, are sufficiently protected by aggregation from efforts towards separation by any mechanical cause?

Since bodies which possess an affinity cannot exist in independently electric states, and since electricity evolved by separation is shown to be a cause insufficient for combination, I see no foundation for Mr. Davy's first assumption,—that heterogenous particles of matter are in differently electrical states; but consider it as irreconcileable to the established laws of electricity and to reason.

The second assumption,—that different particles of matter in combining must still be supposed to retain their peculiar states of energy,—comes next under consideration.

I begin by stating the principal evidence in favour of this supposition. When a compound is decomposed by Voltaic electricity, each component is invariably attracted to a determinate pole. The question stands fairly thus: "Why is the one substance always attracted to a determinate pole, suppose to the negative? Because it is naturally positive. But how is it proved that it is naturally positive? Because it is attracted to the negative pole." There is no service rendered to the hypothesis by this circular reasoning.

I shall now submit the evidence against this supposition.

If

If from the ends of two equally and differently electrified conductors be suspended two gilt pith balls, by gilt strings, the balls will attract each other; but when they come into contact, a mutual annihilation of power takes place; the balls separate, and no longer show signs of electricity.

Or a metallic sphere on which is erected a quadrant electrometer may be insulated and positively electrified, the pith ball rises: if some negative sparks be thrown on, the ball sinks. Were another example necessary of so well known a fact, I would instance two jars differently electrified. Each of them separately produces the most violent effects. It will ignite, fuse and burn the most refractory metals. It will pour out torrents of fire on contiguous bodies, it will instantly deprive animals of life. When a proper communication with the other jar, which is equally charged, is formed, an explosion follows; and instead of either jar being possessed of the additional charge of the other, they are both found harmless and altogether deprived of electricity. All these instances demonstrate in the clearest manner the destruction of forced states, when they come into contact.

As to Mr. Davy's example of Beccaria's plates, it is plain that the cohesion depends entirely on the nonconducting power of the glass; for the opposite powers are thus prevented from coming quickly into contact. The annihilation is effected in some time, and the plates separate. It will be shown, however, that combined particles are in circumstances very different from Beccaria's plates.

If it be proved that matter after combination cannot retain a forced state, the doctrine of combination itself falls to the ground. For bodies in solution which possess affinity should, in the manner of other bodies differently electrified, first combine; and afterwards, obeying any tendency towards separation, as difference of specific gravity, they should separate as soon as the annihilation took place.

It might be imagined that when the bodies are in the fluid state, a separation and consequent evolution of new powers might take place, which would still maintain the compound.

It were unphilosophical to attribute unnecessarily the same natural effects to dissimilar causes. Combination, whether between fluids or solids, may reasonably be pronounced the operation of the same power. There is, however, a stronger objection. The particles of combining substances are surrounded by a conducting medium (water). The moment that different electricities would be produced,

 they

they would be annihilated by the intervention of the fluid.

The objection might offer, that water is not a conductor of low intensities, for instance of the Voltaic. But this does not affect the force of the above reasoning. For, whatever be the intensity supposed to maintain chemical combinations, it is certainly conducted by water; as is proved by immersing after separation any of these substances which become electric by contact, in water. They lose all signs of electricity, and appear in the natural state.

There are other instances wherein the medium is a conductor, and consequently admits not of differently co-existing electricities. All electrics at a high temperature become conductors. Yet many chemical combinations take place at a high temperature only. But how much is this difficulty increased when applied to metallic alloys! the high temperature of which in forming should considerably assist their natural conducting powers.

I scarcely imagine that any one would instance the zones of different electricities producible in a long conductor electrified by a weak power. It must be recollected that with regard to metallic alloys there can be no zones, the whole mass being composed of pairs of atoms united, as it is said, by electric attraction.

In the same manner, so little capable of retaining different electricities is a vacuum which is also a conductor, that, as Beccaria and others have shown, the transmission and annihilation of powers take place insensibly from pith balls, without a change of place in these bodies.

These observations are applicable, whether we consider difference of powers as owing to independent states essential to different bodies, or as always evolved by separation after contact.

The tendency of the preceding observations was to show that Mr. Davy's two assumptions are not only without proof, but entirely in opposition to any thing we know of electricity. I shall now endeavour to show, that even allowing combining bodies to be in different states, and that in uniting they do preserve their peculiar energies, yet still that these powers are inadequate to account for the phænomena attributed to affinity.

If there exist no such power as that which has been called affinity, and if all the phænomena of combination be caused by the differently electrical states of bodies, it should follow that,

1. To produce combination, it is only necessary to present

sent to each other the combining bodies in opposite states of electricity.

2. To counteract combination; or, if formed, to subvert it: all that is necessary is to induce a similar state of electricity in the bodies; or, which is the same thing, to give one a state contrary to that which it naturally possesses.

The latter of these positions Mr. Davy says he has proved by experiment.

In contradiction to these consequences deduced from the hypothesis, I shall produce instances in which,

1. Bodies in opposite states of electricity shall not combine when present to each other.

2. Bodies in similar states shall combine with as much force as if in different states.

And I shall show that Mr. Davy's experiment does not prove its object.

Were the former position of the above consequences true, we should be in possession of an easy method of forming combinations at present believed to be impracticable, as in the same manner substances have been decompounded which never yielded to other means. Thus we should produce all the salts formed by silica with acids; we should unite gold and platina with nitric acid: nay, if there be not that specific attraction attributable to the ultimate particles of heterogeneous matter only, we should combine iron with iron, or potash with potash: and these similar substances held together by different states of electricity should, when placed in the Voltaic circuit, arrange themselves, observing the law of other compounds, according to their respective attractions.

The well established law, that affinity exerts its influence at insensible distances only, ought not to be true; since electricity to exert its attraction does not require an insensible distance. On this account we should also produce all compounds by a mixture of its dry ingredients in fine powder; at least when forced to approach by strong pressure.

[To be continued.]

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 1st and 7th.—The President in the chair. A paper by Mr. Knight was read, detailing experiments on the manner in which plants shoot forth their radicles. In a former

a former paper Mr. K. had proved that gravitation was the chief cause of the descent of the roots of plants; in the present he meant to illustrate, by an account of his experiments, how certain plants extend their roots towards water, and others in a direction from water, without any thing of sensation or animal sensibility, as erroneously supposed by some vegetable physiologists. His experiments established this position sufficiently on mechanical principles, in consequence of the natural inclination to, or aversion from, humidity, according to the particular nature of the plant. He also proved that carrots and parsnips sown in a poor gravelly soil, under which was placed a rich loam, passed through the former, and extended their radicles into the rich loam eighteen inches below the surface.

March 14.—A long and learned paper, by Mr. Baily, was read, on the eclipse of the sun predicted by Thales, as recorded by Herodotus. The author entered minutely into the historical and chronological data which support his opinion, and concluded that the eclipse alluded to was an annular one, which occurred in the year 610 before Christ.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of the Wernerian Natural History Society at Edinburgh, on the 12th of January last, Professor Jameson read the first part of a series of observations on the geognostic relations of the rocks of the island of Arran. In this memoir he described particularly the granite, gneiss, mica-slate, and clay-slate formations, and also the red sandstone, and porphyry-slate, which occur so abundantly in that island. When describing the *granite*, he stated as a conjecture, that quartz might prove to be an older formation than granite, because the oldest granite contains much quartz, but little mica; and less felspar than the newer varieties. He pointed out several observations to be made with the view of verifying or refuting this conjecture. In his description of *gneiss* he alluded to the veins of granitic gneiss which traverse it, and which, when the gneiss and granite are in contact, have been represented as veins of granite shooting from the subjacent into the superincumbent rocks. The *red sandstone* the professor appeared inclined to refer to the first or old red sandstone of Werner. When describing its stratification and structure he pointed out the appearances that ought to be attended to, in endeavouring to ascertain the dip and direction of the strata, and cautioned observers against confounding the structure of individual beds with the direction and dip of the

the strata. The numerous fissures that traverse the sandstone of Arran, and which exhibit every variety of magnitude, direction and dip, afforded apt illustrations of Werner's theory of veins. The *porphyry-slate* the professor described as appearing in the form of overlying conical or irregular tabular-shaped masses, resting on the red sandstone; also in veins traversing granite, sandstone, greenstone, and other rocks. He gave a description of some tabular masses of this rock, accompanied by pitchstone and claystone, contained between strata of sandstone, and which might be confounded with beds, but which he was inclined to consider merely as lateral branches of veins, or as slightly inclined veins.

At the next meeting, on the 2d of February, Professor Jameson read the continuation of his mineralogical observations on Arran. He first detailed the geognostic relations of the *flœtz greenstone* of that island. From this account it appeared to occur in overlying masses resting on sandstone, in beds in sandstone, and in veins that traverse sandstone and other rocks. He next described the various kinds of *pitchstone* that occur in Arran, and seemed to think that one of the varieties might constitute a distinct subspecies of the pitchstone species. The account of its geognostic relations afforded a detail of many curious geognostic appearances; in particular the structure of its veins, and the nature of the interposed tabular masses, having many of the characters of beds, yet appearing to be either nearly horizontal veins, or lateral branches of common veins. The *claystone* of Arran, which was next described, appeared to occur in overlying masses, along with porphyry-slate, and also in veins along with pitchstone and porphyry-slate. It would seem that *wacke* and *basalt* are not very frequent or abundant rocks in Arran; but when they are observed, they present the usual appearances and geognostic relations.

From the observations in these two memoirs it appears, that this island contains no *transition* rocks; but is principally composed of *primitive* and *flœtz* rocks. The *alluvial* rocks that occur in the valleys present the usual characters of the rocks of this class.

XLII. *In-*

XLII. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir, Your Magazine for February assures the public that Dr. Hutton intends to publish a new edition of his Mathematical Dictionary, with the improvements which have been superinduced in the science since the period of publication of the edition of 1795. May I be allowed, through the same medium, for myself and others who possess that edition of his work, to express a hope that the Doctor will also publish the additional information *distinct*, for the perfecting of the copies of that edition which has had so extensive a circulation. I am, sir,

Your most obedient humble servant,

Saturday, March 2, 1811. X. Y.

The third and concluding volume of Mr. Parkinson's Organic Remains of a former World, containing the fossil remains of echini, shells, insects, crustaceæ, fishes, amphibiæ, quadrupeds, &c. with twenty-three coloured plates, will be published in the middle of June.

METEORIC STONE IN INDIA.

A letter from Futty Ghur, in the East Indies, dated 21st July last, presents us with the following imperfect, though curious, account of a phænomenon which has become frequent in Europe and America of late years: "I open this letter to let you know of a very odd circumstance which happened a few days ago, viz. A large ball of fire fell from the clouds, which has burnt five villages, destroyed the crops, and some men and women. The ball is now still to be seen: it is as hard as a stone. This happened near Shahabad, across the Ganges about thirty miles northward from this place. I have heard nothing further about this, but a vague report."

LECTURES.

A Course of Lectures on the Chemical Agencies of Electricity, by Mr. George Singer, will commence on Thursday the 11th of April, at the Scientific Institution, Princes-Street, Cavendish-Square. These Lectures include the most recent Discoveries, and will comprise an experimental Examination of Mr. De Luc's Analysis of the Voltaic Instrument.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To James Hume, of Percy-street, in the parish of St. Pancras, and county of Middlesex, esq. for his sweeping machine or brush, or improvement on a sweeping machine or brush, or sweeping machines or brushes.—February 28, 1811.

To Robert Salmon, of Woburn, in the county of Bedford, surveyor, for certain instruments for the relief of hernia, which instruments he calls Salmon's new royal patent artificial abdomens.—March 4.

To William Southwell, of Gresse-street, Rathbone-place, in the county of Middlesex, piano-forte-maker, for certain improvements in the construction of a piano-forte.—March 4.

To Edward Savage, of Oxford-street, in the county of Middlesex, tin-plate worker, for a machine for washing and bleaching of linen and other articles, and for cooking by means of steam, with a roaster or oven, and warm closets attached, all heated by the same fire.—March 4.

To John Trotter, of Soho-square, in the county of Middlesex, esq. for improvements in musical instruments.—March 4.

To Sarah Guppy, wife of Samuel Guppy, in the city of Bristol, merchant, for her mode of erecting and constructing bridges and rail roads without arches or sterlings, whereby the danger of being washed away by floods is avoided.—March 4.

To William Turner, of 'Change-alley, in the city of London, merchant, for his pike or halbert, with couteaus.—March 4.

To John Plaskett, of Garlick-Hill, in the city of London, stave-merchant, and Samuel Brown, of Norfolk-street, Southwark, in the county of Surrey, cooper, for their method of making or manufacturing of casks and other vessels by improved machinery, which machinery is applicable to other useful purposes.—March 6.

To Thomas William Sturgeon, of Howland-street, in the county of Middlesex, esq. for his improved castors.—March 6.

To Abraham Willis, of Deritend, in the parish of Aston, and county of Warwick, for his new method of producing steel toys of different descriptions, such as barbers' curling-irons, sugar-nippers, snuffers, and other articles.—March 6.

To Richard Jackson, of the Bear Garden, Southwark, in the county of Surrey, iron-manufacturer, for his method or methods of making the shanks of anchors and other

other large bodies of wrought iron of a similar form, by using one solid cove of iron for the centre, with bars of feather-edged iron, so made up, constructed, and applied, as to save a considerable quantity of iron and coals and labour in the manufacturing of the same, and which will materially add to the strength and soundness of all large bodies of wrought iron so manufactured.—March 7.

To John Collinge, of the Bridge Road, Lambeth, in the county of Surrey, axle-tree manufacturer, for his improvement in and upon carriage and other wheel boxes and axle-trees.—March 9.

To James Smethurst, of New Bond-street, in the county of Middlesex, lamp manufacturer, for certain improvements applicable to lamps of different descriptions.—March 11.

To James Mallory, of the State of New York, one of the United States, now residing in the city of London, hatter, who, in consequence of communications made to him by certain foreigners residing abroad, is in possession of a method of making certain machines for cutting and shearing the fur from all pelteries, and for shearing cloth.—March 12.

To Thomas Jones, of Cleveland-street, Fitzroy-square, in the county of Middlesex, carpenter, for his machine for cutting corks and bungs.—March 14.

To Thomas Willis Cooper, of Old-street, in the parish of St. Luke, in the county of Middlesex, millwright, for certain apparatus to be fixed on the naves of wheels and beds of axle-trees of carriages, so as to prevent accidents from the axle-trees breaking; or, if either of the axle-trees should happen to break, the carriage dropping only about a quarter of an inch, the same carriage will proceed on to the end of its journey, without danger of any accident happening from the same; or, if by any means the linch-pins get out or cap-screws get off, the wheels will keep in their former situation.—March 14.

To Robert Davis, of Birmingham, in the county of Warwick, umbrella and parasol furniture manufacturer, for his composition for certain improvements in the manufacturing of all kinds of umbrella and parasol furniture.—March 14.

To George Ferguson, of Barbican, in the city of London, gent. for his lamp with its appendages.—March 14.

To David Stewart, of Stamford-street, in the parish of Christchurch, and in the county of Surrey, architect, for certain improvements in the method of rendering dwelling-houses,

houses, theatres, hospitals, prisons, shippings, conservatories, green-houses, hot-houses, and every other kind of building, air- and water-tight, as far as relates to the glazing, by means of a lap made of copper or any other metal or semi-metal prepared by machinery for that purpose.—March 22.

To Robert Bill, of Rathbone-place, in the county of Middlesex, esq. for his machine or apparatus to facilitate the operation of washing clothes, and other processes necessary in family and other establishments.—March 26.

To Robert Wornum the younger, of Princes-street, Hanover-square, in the county of Middlesex, piano forte-maker, for his improved upright piano forte.—March 26.

To Joseph C. Dyer, of Boston, State of Massachusetts, one of the United States, but now residing in the city of London, merchant, for his new and improved methods of splitting hides, and shaving or splitting leather.—March 26.

To John Craigie, of the city of Bath, and county of Somerset, esq. for his improvements on waggons, carts, and other wheel carriages, whereby friction may be saved, labour facilitated, and a greater degree of safety obtained. —March 26.

To Ann Hazledine, of Bridgenorth, in the county of Salop, widow of John Hazledine, of Bridgenorth aforesaid, engineer, deceased, that she, in consequence of a discovery communicated to her by her said husband, is become possessed of an invention of certain improvements in a plough for the cultivation of land.—March 26.

To John Rose, of Folkestone, in the county of Kent, a lieutenant in our navy, and Thomas Chapman, of Gough-square, in the city of London, gentleman, for their new means of conveying vessels of any burthen through the water without the assistance of oars or sails.—March 26.

To Samuel Kerrod, of Reading, in the county of Berks, plasterer, for his cement and a size for plastering and stuccoing walls, setting and whitening ceilings, and running and whitening cornices, and colours to be laid on the stucco, as well in oils as distemper, the whole of which is intended for the finishing the inside of houses.—March 26.

To James Bell, of Fieldgate-street, Whitechapel, sugar refiner, for certain improvements in the manner of cutting, shaving, or scraping sugar loaves and lumps, and of pulverizing or reducing to small grains or powder, sugar loaves, lumps, and bastard sugar.—March 26.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For March 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 26	46	51°	46°	29·46	30	Fair
27	45	50	41	·67	36	Fair
28	46	51	48	·54	0	Rain
March 1	42	45	40	·64	37	Showery
2	44	52	47	·78	36	Cloudy
3	46	56	45	·88	41	Fair
4	45	57	47	·98	45	Fair
5	46	53	50	·40	40	Cloudy
6	42	50	49	·55	29	Cloudy
7	47	50	48	·20	0	Rain
8	48	48	36	·45	0	Stormy
9	36	45	38	30·30	44	Fair
10	42	53	46	·45	52	Fair
11	44	52	46	·40	32	Fair
12	46	54	45	·38	30	Cloudy
13	40	50	42	·28	44	Fair
14	42	48	40	·30	40	Fair
15	39	45	40	·30	40	Fair
16	38	53	39	·22	62	Fair
17	37	58	41	·14	60	Fair
18	38	58	45	·10	52	Fair
19	47	58	46	·16	42	Fair
20	47	56	50	·08	23	Cloudy
21	49	59	50	29·98	35	Cloudy
22	47	50	36	30·20	30	Cloudy
23	34	51	40	·42	39	Fair
24	41	57	40	·31	42	Fair
25	41	52	41	·11	49	Fair
26	40	55	39	·05	42	Fair

N.B. The Barometer's height is taken at one o'clock.

XLIII. *Description of a portable Mineralogical Laboratory.* By FREDRICK ACCUM, *M.R.I.A. F.L.S. Operative Chemist, and Lecturer on Chemistry and on Mineralogy and Pharmacy.*

SINCE chemistry has in a manner changed its appearance, since its means of analysis have been increased, and since the instruments of research have acquired their present high degree of perfection, new paths for exploring the productions of nature have been opened, the art of experimenting has been simplified, and become more familiar and easy. Most of the operations of chemical science which formerly demanded a regular laboratory, may now be performed on a small scale, with more perspicuity and expedition by the help of such instruments as most persons can command.

Thus, however varied the objects of experiment may be, and however numerous and different the products to be obtained may appear, the most costly materials may be used at a trifling expense in such pursuits. The operator is enabled to observe the gradual changes of each process with more facility and speed in the small way, than in the large; it is in his power to urge, or to retard readily the operation at pleasure, and to ascertain each step of the experiment, from beginning to end.

Such advantages will be valued properly by those who know that the most experienced and most attentive chemist meets with frequent accidents, by which both the vessels and the products of the operations are lost, because he has it not in his power to ascertain the nature of the results as occasion may require. It is thus also that, among the furnaces of the laboratory, many appearances often pass away unnoticed, which are readily observed when the same operation is performed on the table, and under the immediate eye of the experimenter. Besides, most of those investigations which in the large way require several days' labour, can on a small scale be finished in a few hours. The heat of the most violent furnaces may instantly be produced by a stream of air, passing from a blow-pipe bladder, on a piece of ignited charcoal, or through the flame of a candle or spirit lamp. By means of the brilliant flame of the *lamp furnace*, a vast number of chemical operations may be performed, which thirty years ago would have required a series of complex furnaces. All the processes of digestion, the sublimation of salts, the solutions of earthy and metallic

metallic bodies, the concentration of saline and other liquids, the desulphurations of metallic ores, the multifarious processes of distillation by the naked fire, or the sand-bath; the production of gases with the pneumatic apparatus; and even the fusion of earthy fossils with alkalies for analysis, may be accomplished, on the table, by means of this apparatus alone, with much neatness and at a trifling expense.

And as the knowledge of chemistry is founded on practical research, we cannot hope to pursue the study of it with advantage, without performing such processes as verify most of the capital generalities of the science, and also such as reasoning, analogy, and a laudable desire of experimenting, never fail to suggest to those whose taste and talents lead them that way. This mode of study is the more essential, because, in the most common operations of experimental chemistry, a vast number of small facts occur, which are not mentioned in books, but are essential to be known: if they were described as often as they present themselves in practice, a great loss of time would follow, because they are too numerous and too minute, and no advantage would be gained in perspicuity. It is the knowledge of these facts which distinguishes the expert operator from the bungler, and this knowledge can only be acquired by actual practice or manual application; and not from reading, nor public lectures, or other means.

To give effect to operative researches, the most celebrated philosophers have furnished collections of instruments of experiment to facilitate the attainment of practical knowledge. The bare mentioning of some of them will be a sufficient proof of what has been stated. The blow-pipe apparatus of Bergman; the pocket laboratory of Cronstedt; the travelling chest of chemical re-agents of Göetling; the œconomical laboratory of Guyton Morveau; the mineralogical chest for the analysis of soils and manures, lately recommended by the illustrious Professor of the Royal Institution*; and many others, are too well known to render further observations concerning the utility of the above statement necessary.

Emboldened by these proceedings, I presume it cannot be thought foreign to the views of a journal which professes to be consecrated to the diffusion of chemical knowledge, to lay before the public a *sketch* of a Portable Mineralogical Laboratory, which is designed chiefly for those cultivators of mineralogical science, whose means of in-

* Davy's Memoir on the Analysis of Soils.

dulgence

dulgence in experimental pursuits are limited, or who have neither leisure nor inclination to operate in the laboratory. The approbation which this portable collection of instruments has met with among the mineralogical public, and the sanction which I have received concerning it from different quarters, give me reason to believe it has proved useful; indeed I feel no hesitation to say, that the assistance which this collection of instruments and tests is capable of rendering to those who are not without some tincture of science, may promote the diffusion of knowledge and accelerate the progress of the student. It may enable the young mineralogical chemist to acquire readily such information concerning a mineral he may meet with in his travels, or elsewhere, as is sufficient to determine the uses to which the substance may be applied. And it is from a want of mineralogical inquiries, particularly among men of landed property, that useful minerals have often been overlooked; and many valuable products might probably be discovered in situations where they are least expected by landlord or tenant; because a general knowledge of the composition of minerals (or, to speak more correctly, *what* a body contains, and not *how much*,) is usually sufficient to direct their application to beneficial purposes.

The Contents of the Mineralogical Laboratory are the following:

CHEMICAL APPARATUS AND INSTRUMENTS.

A lamp-furnace with Argand's lamp. Fig. 1.

A balance, so constructed as allows it to be used hydrostatically, with accurate sets of weights. Fig. 2.

Crucibles; of silver, porcelain biscuit, and black lead. Figs. 3. 3. 3. 3. &c.

A spirit-lamp. Fig. 4.

A magnetic needle and stand. Fig. 5.

An assortment of flat-bottomed evaporating basons of porcelain biscuit *.

A blow-pipe (Fig. 6.)—A blow-pipe forceps entirely made of platina, a blow-pipe spoon, and platina foil.

Watch-glasses in sizes for evaporating small quantities of fluids.

A copper sand-bath to fit the Argand's lamp.

An apparatus for drying precipitates at certain temperatures, and also for evaporating fluids by steam. Fig. 7.

* The articles named without a figure of reference, cannot be seen in the perspective drawing of the chest.

A hand

A hand mortar of Wedgwood's ware. Fig. 8.
Glass funnels in sizes. Fig. 9.
A cubic inch and ounce avoirdupoise measure. Fig. 10.
Spatulas of steel, silver, and ivory.
Glass retorts and receivers. Fig. 11.
A deep magnifier.
Cylindrical glass tubes, for observing the changes produced in small quantities of fluids, by tests, &c.
An iron ladle for roasting ores, &c.
A steel anvil and hammer.
A collection of matrasses, flasks, and assay jars, in sorts and sizes, for effecting solutions, digestions, &c. Fig. 12.
A steel graver for examining the degrees of hardness of minerals.
Chemical tongs, for removing crucibles out of the fire.
Polished bars of zinc, copper, and iron.
A levigating mortar. Fig. 13.
Glass rods for stirring corrosive fluids.
A piece of hardened steel—Pieces of well-burnt charcoal—A blow-pipe candle—Phials of all sorts—Unsized filtering paper—Strips of bladder for luting, and sundry other small articles.

RE-AGENTS, TESTS, AND CHEMICAL PREPARATIONS.

Sulphuric acid.
Nitric acid.
Muriatic acid.
Tincture of turmeric.
Alcohol.
Borax deprived of its water by heat.
Muriate of gold.
Muriate of barytes.
Liquid ammonia.
Nitrate of silver.
Potash.
Succinate of soda.
Hydrate of barytes.
Muriate of tin.
Muriate of platina.
Oxalate of ammonia.
Acetite of silver.
Tincture of galls.
Tincture of cabbage.
Prussiate of potash.
Phosphate of soda.
Black flux.
White flux.
Muriate of ammonia.
Nitrate of potash.
Nitrate of lead.
Litmus, turmeric and Brazil wood papers.

A person provided with such an assortment of instruments, tests, &c. may perform the analysis of any ore, earth, or other substance of the mineral kingdom. He may perhaps occasionally want a few other articles which are not contained in the chest; but all these may easily and instantly be prepared; and others, such as the common or impure

impure acids and alkalies of commerce, are to be had every where. All the instruments and bottles are arranged in such a manner that they may be seen at one view when the chest is open, and they are so packed that they may readily be taken out; and, when replaced, fit in such a way, that the whole, when the chest is locked, may be turned upside down without risk of receiving injury.

Compton-Street, Soho, March 1, 1811. FREDRICK ACCUM.

XLIV.—*Observations and Experiments concerning Mr. Davy's Hypothesis of Electro-chemical Affinity.*—By M. DONOVAN, *Esq.*

[Concluded from p. 233.]

WHEN water is decomposed by means of metallic wires, which have an affinity for oxygen, the wire conveying positive electricity becomes oxidated. When a platina wire is employed, it does not oxidate. But by the hypothesis the union ought to take place; for the platina, naturally positive, has its energy considerably exalted, and consequently should unite with as much force to negative oxygen as any other metal under the same circumstances. The matter is simply thus: The cause of combination is attraction, the cause of attraction is existence in differently electric states: the more energetic these states are, the more violent is the attraction. Mr. Davy's words on this part of the subject are: "As the chemical attraction between two bodies seems to be destroyed by giving one of them an electric state different from that which it naturally possesses, that is, by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy. Thus, while zinc, one of the most oxidable of the metals, is incapable of combining with oxygen when negatively electrified in the circuit, even by a feeble power; silver, one of the least oxidable, easily unites to it when positively electrified; and the same thing may be said of other metals." In the instance present, the oxygen is in the negative state, the platina is strongly positive, and precisely in the same circumstances as the silver in Mr. Davy's instance. Why then do they not combine with violence*?

It

* In the Philosophical Magazine, vol. xxxiii. p. 88, we find the following, from a Correspondent:

"Mr. Davy showed, by a refined application of his principles, that, in the decomposition of a neutral salt in solution, the order of the arrangement varies.

It would seem as if this one fact were sufficient to establish a decided difference between chemical affinity and electro-chemical attraction; since the former is absent, where the latter is present in an eminent degree.

Pursuing this reasoning, we find that, at the other wire, hydrogen is evolved. Mr. Davy has shown that the least oxidable metals easily unite to oxygen when their opposite states are exalted. Such is the case with silver. Now since silver in its natural state has little attraction to oxygen, and since in its exalted state it easily unites, is it an unfair conclusion, that, if made strongly negative, it should unite also to hydrogen? for, as the cause of combination is the same in both, we ought to obtain the compound of silver with hydrogen as well as that with oxygen. If there be any difference in the circumstances, it is that silver has naturally some attraction to oxygen, and none to hydrogen; which would cause the combination with the former to be somewhat more quickly formed than that with the latter.

Having now shown instances of bodies in different states, not combining, I proceed to the next position, that "bodies in similar states do combine."

With a view of ascertaining by direct means whether bodies in similar states of electricity do combine, I made the following experiment.

Two cylindrical vessels (Fig. 1, Plate V.) one of glass, the other of metal, are connected by a stop-cock in such a manner that the cock, when open, allows all the fluid of one vessel to flow into the other. The vessels are insulated, and have each a wooden stopper, through which a conductor forms a communication between an electric machine and the fluid within. There are quadrant and pith-ball electrometers, the ends of which also plunge in the fluid. The use of the conductors is to throw in an electricity, which must in both be of the same kind, as it is furnished from the same source; but to prove it beyond doubt is the use of the pith-balls, and can readily be done with sealing-wax in the usual manner. The quadrant electrometers in-

varies. When copper wires, which readily combine with oxygen and are easily soluble in acids, are used to transmit the electricity, the positive wire attracts the oxygen and acid, and repels the hydrogen and alkali. But when platina wires are employed, which have but a very slight affinity for oxygen, [are not electric attraction and affinity the same?] the phænomenon is very different. Oxygen and acid, as before, are attracted by the positive pole; but as they are incapable of uniting to the platina, [why?] they instantly receive by contact its electric state, and exercise a repulsive power towards it: the same effect takes place with the hydrogen and alkali at the negative pole."

This philosopher seems to have overlooked a position manifestly in contradiction to the hypothesis which he intends to support.

dicate

dicate the equal intensities of the powers thrown in. The vessel *a* is made of metal, lest it should be thought that the change resided in the surface if made of glass, and contains a quantity of oxalic acid in solution. The other vessel *b* contains solution of lime, which being naturally positive admits the use of glass for the convenience of seeing the result. The stop-cock is furnished with a long insulating handle, and the whole is set on an insulated stool. Matters being in this state, a full stream of positive electricity was poured in from a very powerful machine, through the two conducting wires. After the pith-balls were proved by wax, and the quadrant electrometers observed to stand the same number of degrees, the stop-cock was turned by its glass handle. On the mixture taking place, the oxalate of lime immediately precipitated.

Having emptied and dried the apparatus, the whole was arranged as before. The metallic vessel contained solution of oxalic acid, the glass-vessel was empty. A stream of electricity was poured into the solution, and the stop-cock meanwhile opened: when the fluid touched the wire of the pith-balls in *b*, the pith-balls immediately diverged with positive electricity; clearly demonstrating that the acid solution had carried with it positive electricity.

In the experiment the agency of negative electricity cannot be suspected. If any had been produced by the contact of the two solutions, it must have been immediately destroyed by the constant streams pouring in from the machine. I made use of oxalic acid and lime, these substances being instanced by Mr. Davy as remaining in different states when separated.

Oxygen and substances in which oxygen predominates, as acids, are attracted to positively electrified surfaces, and are repelled by surfaces negatively electrified. These bodies are therefore themselves in the negative state. Now if for example phosphorous acid be negative and oxygen in the same state, why have these substances so strong an attraction, and why do they combine to form phosphoric acid? It might be said that the phosphorus in the acid being positive, although combined with oxygen in the negative state, but not to saturation, yet has an excess of positive electricity, and that this excess attracts the additional quantity of negative oxygen. Were this the case, phosphorous acid with its positive excess should be repelled by the positive and attracted by the negative pole; which is not only contrary to fact, but contrary to the hypothesis.

 I can

I can also produce instances of bodies neutral* with regard to attraction, which combine with the most intense force. Potassium has an amazing attraction for oxygen; their combination is quick, and the heat and light intense. The result of all such powerfully energetic attractions is a compound which is neutral as to other bodies. Such a body is also water. Here then are two neutral bodies; and yet so powerful is their affinity for each other, that in ordinary processes we never obtain potash free from water.

Mr. Davy seems to place great reliance on the fact that a copper wire which is naturally positive, when made negative, will not be acted on by nitrous acid, which is naturally negative. This however proves nothing; for it does not follow that it is electric repulsion which prevents the combination: and if it did follow, it would be far from proving that combination is caused by electric attraction.

Having now given examples, in which combination ought to take place, and does not, as well as instances in which combination does take place, and ought not; I shall proceed to some general remarks.

There are, I think, a variety of facts inexplicable by the agency of electricty, which are easily accounted for if attributed to affinity: for instance, the various attractions coexisting in certain salts. Let the example of super-phosphate of potash be selected, the component principles of which have been already mentioned. Positive phosphorus unites to negative oxygen, forming phosphorous acid; and this must be supposed to be still positive, as it unites with another dose of negative oxygen, forming phosphoric acid. Positive potassium combines with negative oxygen, forming potash: this being positive has an attraction to the negative phosphoric acid: they combine, and form phosphate of potash; which must be still positive, as it has another attraction to an excess of acid: this additional dose enters into union, and at length there is formed super-phosphate of potash, which has also an attraction to water, forming

* "Similar effects may be conceived to occur in the case of oxygen and hydrogen, which form water, a body apparently *neutral* in electrical energy to most other substances: and we may reasonably conclude that there is the same exaltation of power in all cases of *combustion*. In general, when the different energies are strong and in perfect equilibrium, the combination ought to be *quick*, the heat and light *intense*, and the new compound in the *neutral* state. This would seem to be the case in the instance just quoted, and in the circumstances of the union of the strong alkalies and acids. But when one energy is feeble and the other strong; all the effects must be less vivid; and the compound, instead of being neutral, ought to exhibit the excess of the stronger energy."—*Davy's Bakerian Lecture.* Phil. Trans.

the

the crystals of this salt: and lastly, the water itself is a compound of oxygen and hydrogen, united by a very powerful attraction.

Here are no less than fourteen electric attractions forming seven combinations, all operating quietly together. Is it possible, from any thing we know of electricity, to form a definite idea of so many complicated powers; so nicely balanced too, that in some instances the smallest touch, nay, the friction occasioned by falling through air, is sufficient to make them arrange themselves in a new order, and the change is attended by the most violent effects?

By admitting that these combinations are caused by an attraction *sui generis* called affinity, which is an essential property of matter, the explanation becomes extremely easy. But the difficulty of the other is not surmounted by supposing with Mr. Davy that electricity is no more than a property of matter. For, beside that the hypothesis is inadmissible, as shall be presently shown, we know that the facts are objects of sense convincing and unalterable, whatever conceptions we may have of that power which occasions their production.

I shall now state my reasons for affirming that electricity is a fluid *sui generis*. If electricity be a property of matter, it ought to be inseparable from matter; we can have no clearer conception of a property abstracted from matter, than we can of colour independently of the body coloured. If it be shown that it is separable, it necessarily follows that it is an absolute substance; as the moment it is separated it ceases to be a property. That we do obtain it in an insulated state is, I think, shown by experiments with the Torricellian vacuum. Do we not see streams of electricity pervade the vacuum*? Can we not detect it in its progress by its effects? It is clearly shown also by holding a quire of paper in the interrupted circuit of a battery. On making the discharge, the paper is perforated with violence; and we see a prodigious volume of condensed electricity. What has done this? Is it done by that which has no more existence *per se* than solidity, extension, or figure, have without substance; which is no more separable from matter, than splendour, tenacity, ductility, from metals? If it be not a substance, what causes the smell and taste so apparent from electrified points? It is not caused by particles

* If air be entirely absent, it is true the light is much less perceptible, if at all; but that electricity still passes, is proved by the divergence of balls connected by conductors with the vacuum.

driven

driven off from the points, so small in quantity as not to have appreciable gravity, as some have asserted; for the smell and taste are the same, whatever be the substance from which they issue. Lastly, I would know what is it which diffuses itself from one conductor over the surface of another, when separated by a great space; which in fine affects the organs of sensation with every effect of materiality.

It is pretty certainly known that caloric enters into chemical combination with bodies; and that it is a really chemical combination is proved by the mutual change of properties consequent on the union. Thus ice combining with free caloric forms water; the ice has lost its solidity, the caloric is become insensible. Are we to suppose that caloric is in a state differently electrical from that of water? The powerful attraction must be all on the side of the caloric, as water is said to be neutral with regard to other bodies.

The hypothesis gives no satisfaction concerning the separation of oxygen from various bodies by light. Is this substance also possessed of electric energy?

It is scarcely possible to conceive how the firm combinations with which we are acquainted, can be occasioned by so weak an attraction as that of electricity; and the less so when it is considered how very small is the quantity of this power apparent in these very experiments which gave origin to the hypothesis of electro-chemical attraction.

The contact of very large surfaces almost always requires to be made several times before the gold leaves of Bennett's electrometer are sensibly affected. Every one is acquainted with the amazing sensibility of this instrument. What must be the effect of a single contact of one pair of atoms? Yet it is certain that the attraction of one pair of atoms is of as great intensity as the combined powers of all, however great the quantity. The difficulty is considerably increased, if we suppose that the electricities of heterogeneous bodies exist in an absolute state. They must then be so feeble as not to be appreciable by the most delicate instruments in our possession; beside that, in the latter case, the agency of electricity must be entirely supposititious. The following experiment of Mr. Davy's, I think, tends to confirm my objections against the efficiency of electric agency.

Mr. Davy heated together a plate of copper and a plate of sulphur. The electricity which was scarcely sensible at 56°, even to a condenser, became only powerful enough at 100° to cause a divergence of the gold leaves without con-

condensation. They increased in a higher ratio as the sulphur approached its point of fusion; at a little above which these bodies combine with the evolution of light and heat.

The electricity at 100° was barely sufficient to make the gold leaves diverge. How extremely low must have been its intensity! for it is certain that the leaves of Bennett's electrometer diverge with almost any change in the surrounding media. Thus, if powdered chalk be blown from the nosle of a bellows upon the brass cap, the leaves diverge; or it is only necessary to let the chalk powder fall on the cap. Can it be supposed, although the electricity increased in a ratio somewhat higher towards the melting point of sulphur, which is 226°, that so low an intensity, as it still must be, could cause a combination attended by such a violent extrication of heat and light?

I also have made some experiments on the contacts of different substances. A plate of insulated copper and a plate of glass were heated to about 130°. When separated, each caused a divergence of the gold leaves. The electricity was always weak. Once, when the sun shone very strongly, the electrometer had acquired exquisite sensibility, and under these circumstances the divergence was somewhat considerable. I never afterwards succeeded so well.

A plate of sulphur and a plate of glass, when heated and afterwards separated, caused a very sensible divergence of the gold leaves. When the glass on one side was coated with tin foil equal to the diameter of the sulphur, and heated as before, the leaves diverged nearly half an inch. These experiments exactly coincide with some made by Mr. Wilcke in a different manner. This philosopher found that, when sulphur was melted, and allowed to solidify in glass vessels, they both acquired a strong electricity; but that the electricity was much stronger if the glass were coated with metal.

I repeated Mr. Wilcke's experiment with some little variation. I poured melted sulphur on a *plane* of glass, and cemented on an insulating handle. When solid, its electricity was so strong that it attracted large pith-balls as vigorously as if excited by strong friction. The intensity of the glass was much lower. These experiments of melted sulphur do not differ from that of the heating of sulphur made by Mr. Davy, otherwise than that the former mode is more decisive; a complete contact is formed, and the heat is general and equal, and accordingly the results are less equivocal.

Æpinus

Æpinus found that when two plates of glass, such as are used for looking-glasses, were pressed together and afterwards separated, they acquired a strong electricity, but different in each plate.

These experiments can be only performed under certain circumstances of the atmosphere, as when the air is dry and the sun shining strongly.

If Mr. Davy's experiment be sufficient to ground the supposition that sulphur and copper combine by the attraction of their different electricities, I have the same grounds for supposing that copper and glass, sulphur and glass, or glass and glass, unite chemically; since by contact they produce different states of electricity.

Having made a statement of the principal objections which occurred respecting the electro-chemical doctrines of combination, I shall now proceed to notice whatever remains on decomposition.

The manner of decomposition in general has been already noticed. I am now prepared to enter on this branch of the inquiry more minutely.

Mr. Davy has, in different parts of his writings, given two modes in which decompostion is effected, each of which I conceive to be essentially different from the other.

1. That the electricity of each pole attracts that principle of the compound which is an opposite state of itself, and repels the principle which is a similar state; and that this happens at both poles.

2. That the electricity of each pole, where it is in contact with the compound, brings the component principles into similar states, and that they consequently repel each other.

I shall endeavour to show that decomposition cannot be produced according to either of these positions. This I hope to accomplish by proving,

1. That the interchange of electrical powers ought not to cause electro-motion in the principles of the compound; and, allowing electro-motion, that combination and not decomposition ought to take place.

2. That, if the decomposition of combined bodies be caused by the repulsion of similar states artificially acquired, both principles of the compound ought to be found in a separate state collected round the polar wires.

Beginning with electro-motion, it is evident that, in the solution of a salt, we must suppose a number of particles surrounded by water. That water is a conductor of that electricity

electricity which is supposed to cause combination has been already shown. Whether it be a conductor of such intensities as the Voltaic, is perfectly immaterial to the inquiry. When in the saline solution are immersed the polar wires, the latter as usual exert their attractions in concert to the opposite electricities of the saline elements. It becomes a question, Why do not the electricities of the decomposed elements pass through the fluid conducting medium towards the poles, without carrying also the elements with which they were combined? for the original electricity of the elements is immediately annihilated in its passage towards the poles. Consequently all attraction between it and the element with which it was combined, must be destroyed. Besides, in all our experiments, we find that to produce attraction or repulsion it is necessary to have an electric interposed between the body acting and the body acted on.

It is next to be examined how far decomposition can be effected in the manner stated.

It is an axiom, that a force cannot be overcome by a force which is not greater. Then, if a quantity of artificial electricity thrown into a compound occasion decomposition, that electricity must be possessed of superior intensity. We will suppose it the positive power thrown into solution of sulphate of potash. The first effort of the superadded power will be to attract the negative acid, which will become saturated or neutral. The second effort will be to combine with, and to exist sensibly and absolutely in, the neutral acid. Thus the acid which was, in the combination, negative, is now positive. The alkali suffers this change conversely, and becomes negative. We have now all the conditions as they primarily existed for producing combination; with this difference only, that the attractions of the bodies are by far more intense; for, if otherwise, the original combination would never have been broken. Why then do they not combine with increased force? And why are they attracted to these very poles, which being in similar states should violently repel them? Independently of increased intensity there are two powerful causes operating to favour combination; one only of which tended to sustain the original salt. First, the bodies are forced to approach directly by their reciprocal attractions; and indirectly by the strong repulsion of the similarly electrified poles. Consequently they should pass to, and occupy the situation in, the fluid where there is least resistance; namely, the mid dle point, where the repulsion of one pole ends and the attraction

attraction of the other begins. This new combination, if not permanent, ought at least to be sustained while the causes continue to exert their influence.

Let us apply these objections to the decomposition of water by Voltaic electricity. When the polar wires are immersed in a vessel of water, the positive wire attracts the negative oxygen, and repels the hydrogen; the negative wire attracts the positive hydrogen, and repels oxygen. During these repulsions, the negative oxygen and the positive hydrogen must meet and unite, again forming water. There are also oxygen and hydrogen attracted to their separate poles. Here each must immediately acquire a state similar to that of the polar wire, in consequence of which each must be repelled; and the course of the gas during its repulsion must be directed by the attraction of the opposite pole. This repulsion taking place at each pole, the gases, oxygen and hydrogen, strongly electrical in different states, should in their attempts to cross, meet and unite; for their power of combination is considerably increased: and thus water should be recomposed.

But if by any ingenuity it can be shown, that they ought not to unite, it ultimately comes to this, that the gases will cross each other, and will pass to poles which, being in different states, will attract and afterwards repel them; and thus a continual series of attractions and repulsions will follow—precisely in the same manner as a suspended pith-ball will continue to play between two jars differently electrified, attracted to one, repelled from that and attracted to the other, so long as there remains contrariety of power in the jars. This example is applicable by a direct analogy. But whether the gas do or do not combine, a bubble ought never to be discharged in a sensible state.

With a view to ascertain whether or not water is recomposed at the central point, as supposed by Mr. Davy*, I made the following experiment. (See fig. 2.)

A glass tube filled with dry powdered muriate of lime, through which passed a platina wire hermetically sealed at

* "The oxygen of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hydrogen, is repelled by it; and the opposite process takes place at the negative surface: and in the *middle* or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter, and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel."—*Davy's Bakerian Lecture.*

both

both ends, was placed horizontally on a small glass pillar; the ends of the platina wire projected beyond the extremities of the tube, and terminated in two small hooks. From these hooks on each side were suspended small bell-glasses, in each of which was contained a platina wire sealed at the top; the latter wire passed through the bell, and formed the connexion with the hooks. The lower ends of the bells, which were open, were immerged in small glasses, and both bells and glasses were filled with distilled water. The conducting wires which proceeded from the battery were armed with a slender piece of well-burnt charcoal. Each piece of charcoal was plunged in the water of the glasses;—so that the electricity was conveyed from the charcoal to the wire in the bell. Here the decomposition commenced. The electricity was conducted through the wire in the horizontal tube; from thence to the wire in the second bell, where another decomposition took place, and at length to the other piece of charcoal. [See the Plate.]

By this arrangement the middle point, where Mr. Davy says water is recomposed, was muriate of lime, which would absorb any water that might be formed, and here it might reasonably be expected to be found.

Having ascertained the exact weight of the horizontal tube and its contents, I connected to the apparatus a battery of 100 pairs of 4-inch plates. The battery was kept in a state of activity for four hours. At the end of this time oxygen was found in one bell, and hydrogen in the other. But the horizontal tube was precisely the same weight as before, notwithstanding that a considerable quantity of water had been decomposed.

The positive wire attracted the negative oxygen, and repelled the positive hydrogen, through the wire and muriate of lime; where meeting with negative oxygen repelled from the other bell, the two gases, being in different electrical states, must have, as Mr. Davy allows, united to form water. As the horizontal tube gained no weight at the end of the experiment, is it not a sufficiently well warranted conclusion that no water was formed, and that consequently the theory given for the evolution of pure gases must be erroneous? These gases, if conveyed through the wires, must have passed through no less than four air-tight sealings. Why should these gases pass, and air be detained?

We now come to the second position, That decomposition is produced by bringing one of the combined substances into a state different from that which is natural to it.

When a particle of a compound has its elements thus brought

brought into similar states, it is certain that they must repel each other. But it is equally certain that they must both be repelled from the polar wire, for it is also in a similar state. The elements should now be attracted over to the other pole. The same thing taking place at the latter pole, the elements there separated ought in the same manner to be attracted to the opposite pole. During the whole, the repulsion of the one pole is aided by the attraction of the other. Applying this to a particular example, suppose to sulphate of potash; we know that potash is naturally in the positive state: when the sulphuric acid is rendered positive by the electricity thrown in, the two substances will repel each other; but both will be repelled from the positive and attracted to the negative wire. The same change is produced by the negative wire; in consequence of which, acid and alkali will be attracted to the positive wire. Thus we should have both elements in a separate state collected round each of the polar wires. If this be followed up, we shall find that the two bodies cannot rest for any length of time at either pole, constantly acquiring similar states;—on which account an endless series of attractions and repulsions will ensue. These effects happen, if the substances in different states be supposed to pass each other without union. If they do not pass, the two substances should combine, and neither acid nor alkali should be found permanently at either pole.

It has been shown that Mr. Davy's first assumption of bodies combining still retaining their peculiar energies, is unfounded; and that after combination there no longer remains any electricity. There is then no reason why electricity thrown in should exert an electric attraction to the one, more than to the other element; or why a sensible electricity may not be diffused over a particle of a compound, without causing a repulsion of its elements. The repulsion should rather be supposed between the ultimate particles of the compound, than between those of its elements.

It now only remains to notice the insufficiency of Mr. Davy's explanation* of the influence of quantity on decomposition. Selecting the instance of the partial decomposition of sulphate of soda by muriatic acid, let us examine how far this could be produced by electric attraction. Muriatic acid and soda are held together by an affinity which

* "For the combined effect of many particles possessing a feeble energy may be conceived equal, or even superior, to the effect of a few particles possessing a strong electrical energy."—*Davy's Bakerian Lecture.*

is

is overcome by that of sulphuric acid; sulphate of soda is formed. From this it appears that sulphuric acid, being more negative with regard to soda than muriatic acid is, exerts a more powerful attraction, and with this attraction exists in sulphate of soda. When muriatic acid is added, which has a weaker energy, it is impossible to conceive how the weaker *electrical* energy could displace the stronger. The soda possesses as strong an attraction to the sulphuric acid as the latter does to the soda. Here there are two attractions, either more powerful than that which is supposed to break the combination. Were the soda in a free state, its electricity might be saturated by a great quantity of a weak power. But it were absurd to suppose that the soda would separate in order to unite with a weaker electricity for which it can have no attraction, being already saturated with the opposite stronger power of the sulphuric acid. The attractions of electricity obey intensity, not quantity; and it may be proved by a decisive experiment. This law alone is sufficient to establish a difference between electric attraction and affinity.

The intensity of electricity is in the inverse ratio of the surface charged compared with the quantity. If a battery and a small jar be electrified with the same quantity of fluid, the intensity of the jar will be to that of the battery inversely as the superficial contents of the former are to those of the latter. Thus if the jar be = 1, and the battery = 3, the quantity of fluid = 6 in each; thus the intensity of the jar will be = 6, and that of the battery = 2, or three times greater. This superior intensity, although not the ratio, may be easily shown by attaching electrometers to the jar and battery. When the quantities thrown in are alike, the electrometer of the jar rises to its maximum, while that of the battery is not affected. The application of the experiment now becomes extremely easy, and its force manifest.

Let a jar be charged by a certain number of revolutions of an electric machine; when removed, let a battery receive the quantity produced by an equal number of revolutions. The ball of the battery is to be placed at some distance from that of the jar, and midway between them is to hang a gilt pith-ball suspended by a gilt thread from a negatively charged jar above. The pith-ball may be confined in its position by means of a silk thread extended by a hand underneath. When the silk thread is let loose, the gilt ball will be attracted to the jar. The same thing happens if the battery contain twice, or perhaps twenty times

the charge of the jar. The battery represents muriatic acid, the positive jar represents sulphuric acid, and the negative jar represents soda. The powers are reversed; but it does not affect the conclusion. These experiments may be made also by employing large and small conductors.

Conclusion.

If I have been thus free in stating my objections to the opinions of one of the most distinguished philosophers of the age, I was encouraged to proceed, when I reflected that, as the establishment of truth was the object of research, the discovery of error as a preparation, would be to no one more highly pleasing, than to the illustrious framer of this ingenious hypothesis.

XLV. *An Analysis of Fluor-Spar.* By THOMAS THOMSON, *M.D. F.R.S.E.**

THE mineral called Fluor-Spar has been long known, and valued on account of its beauty and the ease with which it can be turned on the lathe into various ornaments and useful utensils. It occurs chiefly in veins, and very frequently accompanies lead-ore. Some of its properties have been described more than a century ago; as, for example, its phosphorescing when heated, and its corroding glass when mixed with sulphuric or nitric acid. But it is not forty years since its composition was discovered by Scheele, who demonstrated that it is composed of lime and a peculiar acid called fluoric. Chemists now distinguish it by the name of fluate of lime.

Hitherto, no chemical analysis of this salt has been published, except a very incorrect one by Kirwan and Gren, which has been ascribed to Scheele, though I cannot find it in any of his dissertations on fluor-spar. By that analysis, it is made to contain 27 per cent. of water,—a proportion very inconsistent with the properties of native fluate of lime, which, when strongly heated in a wind furnace, loses at an average only $\frac{1}{600}$dth part of its weight. The obvious inaccuracy of the analysis given by the authors just mentioned induced me to make a set of experiments on it last summer (1807). I selected the purest transparent colourless crystal, which I found by repeated trials to be very nearly pure fluate of lime. When reduced to a fine powder,

* From the Wernerian Transactions.

and

and digested in nitric acid, I found in the acid only a little lime, owing doubtless to the partial decomposition of the fluate, and minute traces of iron and lead: these two metals I detected by evaporating the nitric solution to dryness, heating it to redness, and then dissolving the residue in muriatic acid. The colour of the solution showed the presence of iron, and a few needle-form crystals of muriate of lead were deposited after the solution had stood for some days. The fluate which I used had been dug out of a lead-mine in Northumberland, and small crystals of sulphuret of lead were here and there to be seen in it. Probably some one of these had escaped my attention, and, by being mixed with the fluate which I used, occasioned the appearance of the lead, which, however, did not amount to $\frac{1}{100}$dth part of the salt, and therefore could not occasion any sensible error in the subsequent analysis.

I first tried to decompose the fluate of lime, by fusing it with twice its weight of carbonate of potash in a platinum crucible. Only a small portion of the fluate was decomposed. I was therefore obliged to repeat the fusions very often, washing off the alkali after each operation, by means of water, and then dissolving the carbonate of lime formed, in muriatic acid. Fatigued with the tediousness of this method, and despairing of an accurate result from the great number of successive solutions, I abandoned it altogether, and adopted the following method, much more expeditious and equally precise.

From a mass of fluor-spar which I had ascertained to contain no sensible portion of foreign matter, I separated 100 grains, which I reduced to powder, and digested for some hours in a platinum crucible, with rather more than an ounce of pure concentrated sulphuric acid. The mixture was then evaporated to dryness, and the crucible exposed for an hour to a strong heat, in a wind-furnace. To ensure the complete decomposition of the fluor, the mass was reduced to powder, and treated a second time in the same manner with another ounce of sulphuric acid. The residue, which was white with a slight shade of red, proved on examination to be pure sulphate of lime. It weighed 156·6 grains.

Now it has been ascertained by the most careful experiments, that sulphate of lime thus violently heated contains 43 per cent. of lime. Of consequence, the whole quantity of lime in 156·6 grains of sulphate is 67·34 grains. This is obviously the whole lime contained in 100 grains of fluor-spar; and since that mineral contains no sensible

 portion

portion of water, the remainder of the 100 must be fluoric acid. Hence fluate of lime is composed of,

Lime..............	67·34
Fluoric acid	32·66
	100·00.

This result differs very materially from the analysis alluded to in the beginning of this paper, which makes the amount of the acid in the same weight of spar only 16 grains, and the lime 57 grains; the remaining 27 grains being considered as water. But, from the care with which my experiment was conducted, I flatter myself that the result which it exhibits is very near the truth*.

XLVI. *On the Gaseous Combinations of Hydrogen and Carbon.* By Thomas Thomson, *M.D. F.R.S.E.*†

The gases formerly distinguished by the name of *heavy inflammable airs*, which are evolved during the distillation of most animal and vegetable substances, differ so much from each other in their properties, that it has been hitherto impossible to reduce them under a small number of species. When burnt, they all yield carbonic acid and water, and of course contain carbon and hydrogen; but the proportion of these bodies formed, and of oxygen consumed, during the combustion, varies very much at different times. Many of these gases have been examined with much address by Cruickshanks, Dalton, and Henry. Berthollet also has examined them at different times, and published very ingenious dissertations on their composition.

From a great many experiments on these gases, at different times, and in different states, I have convinced myself that they usually hold an empyreumatic oil in solution, and that the differences in their specific gravity, and other properties, depend very much upon the proportion of oil present. Hence no pure gases, fit for examination, and comparable with each other, can be obtained from those vegetable or animal substances which yield an oil when subjected to heat, as is the case with most of them. To this oil is to be ascribed the great variation in the specific gravity

* Since the preceding paper was read, I have seen an analysis of fluor-spar by Klaproth, in the 4th volume of his Beiträge, conducted in a very different manner from mine, but leading to almost the very same result. He found fluate of lime composed of 67½ lime, and 32¼ acid,—quantities which coincide with mine within less than 1 per cent.——Nov. 1809.

† From the Wernerian Transactions.

gravity of the gas obtained by distilling pit-coal, as shown by the experiments of Dr. Henry.

When water and carbon are present together, as is the case in most animal and vegetable substances, they act on each other, and give origin to variable quantities of carbonic oxide, which must also very much alter the properties of the gas evolved. To these two causes, namely, the oil dissolved, and the carbonic oxide formed, most if not all the varieties in the combustible gases, obtained from animal and vegetable substances, are owing.

Berthollet, in a dissertation which he lately published in the second volume of the *Mémoires d'Arcueil*, has endeavoured to prove that all the heavy inflammable gases contain oxygen as a constituent. He has examined a number of gases obtained by distilling charcoal, and has shown that each of them contained a considerable proportion of oxygen. This opinion has likewise been maintained by Mr. Murray, in his System of Chemistry.

If any confidence can be put in the preceding observations, it is clear that this obscure subject can never be elucidated by examining gases from charcoal, or from animal and vegetable substances. The first will always yield carbonic oxide as well as carburetted hydrogen, and the gas from the other bodies will be disguised by the oil held by it in solution.

Analogy is strongly in favour of the common opinion, that there exists a gas composed simply of hydrogen and carbon. Hydrogen, we know, is capable of dissolving every other simple combustible, sulphur, phosphorus, and boracium. It dissolves likewise several of the metals; as arsenic, zinc, tellurium, and potassium. Why then may it not be capable of dissolving carbon?

There is a gas which rises in considerable quantity from stagnant waters during the summer season. It was examined by Cruickshanks and Dalton; and both of them concluded it to be a compound of carbon and hydrogen, without any sensible quantity of oxygen. But as neither of these gentlemen has published a detailed account of their experiments, I thought it worth while to examine the gas anew, in order, if possible, to determine the point in a satisfactory manner. I accordingly collected considerable quantities of it at different times in the neighbourhood of Restalrig, where ponds of muddy water are left stagnant in order to collect manure. This gas I found to have the following properties:

1. It is colourless, and transparent like air.

2. It has no smell, and no sensible taste, provided it be previously washed in clean water.

3. It always contained a mixture of carbonic acid. The least quantity of this gas present was 5 per cent.; the greatest, $7\frac{1}{2}$ per cent.

4. It always contained a mixture of common air. It is remarkable, that the proportion present was in every case the same, and amounted to 12·5 per cent., or 2·5 per cent. of oxygen and 10 per cent. of azote. Mr. Dalton says, that the gas which he examined contained 20 per cent. of azote. This was never the case with the gas from Restalrig; but the common air and carbonic acid gas together sometimes amounted to 20 per cent., and always to near that quantity.

5. After depriving it of its carbonic acid, I found its specific gravity 0·611, that of air being 1·000. But as it contained a mixture of 12·5 per cent. of air, it would have been obviously lighter, if this portion had been removed. By calculation I find that the specific gravity of the pure gas would have been 0·5554 *.

My method of taking the specific gravity of gases being extremely simple, yet precise, I think it worth while to describe it in this place. It is founded on the well-known fact, that when two gases are mixed, their bulk does not alter. I have a large flask fitted with a stop-cock. I weigh this flask very accurately, then exhaust it, and weigh it again. Let the loss of weight be a. I then fill the flask with the gas whose specific gravity I want, and weigh it again. If the weight of the flask be the same as at first, it is obvious that the gas has exactly the specific gravity of common air. If it be heavier than at first, the gas is heavier than common air; if lighter, the gas is lighter than common air. Suppose the flask lighter than at first, I add weights till the flask is exactly poised. Let the weight added be b. We have the weight of common air to that of the gas as a to $a-b$. And to find the specific gravity, we have this proportion, $a : a-b :: 1 : x =$ specific gravity wanted, or $x = \frac{a-b}{a}$. If the flask is heavier than at first, weights are added to the other scale, till it is exactly poised; let these weights, as before, be b; we have in this case $x = \frac{a+b}{a}$. In this process no correction is necessary

* Let $x =$ the specific gravity of the pure gas, A = the quantity of air in the mixture, $a =$ the specific gravity of air, B = quantity of pure gas present, $c =$ specific gravity of the mixture, we have $x = \frac{(A+B)\,c - Aa}{B}$.

for

for temperature, nor the state of the barometer; because air and all gases undergo the same change of bulk, by changing the temperature and the pressure. The gas is always weighed, saturated with moisture. This is corrected when requisite by the table of Dalton. In this way the flask may be easily weighed, true to the $\frac{1}{100}$dth part of a grain; so that you are always sure of the specific gravity of the gas, to the 3d decimal figure. The specific gravity of common air is always taken 1·000, and at the temperature of 60°, barometer at 30 inches, 100 cubic inches of it weigh 30·5 grains. These data enable us to determine the weight of 100 cubic inches of any gas whose specific gravity is known. Mr. Dalton found the specific gravity of this gas 0·600. I took its specific gravity more than once, and collected at different times, but never found it heavier than 0·5554.

6. The gas from stagnant water burns with a yellow flame, more readily, and with a larger flame than any other combustible gas, except olefiant gas, and the vapour of ether.

7. When mixed with oxygen gas, and kindled by an electric spark, it detonates loudly, and undergoes a considerable diminution of bulk. Its combustibility by this process is confined within very narrow limits. It does not burn unless the bulk of the oxygen rather exceeds its own bulk, and it ceases to burn when the oxygen gas is more than 2¼ times the bulk of the inflammable gas. The limits of combustion, according to my trials, are 100 measures of inflammable gas, and 105 or 227 measures of oxygen gas. A mixture of 100 measures of inflammable gas, with 282 or 292 measures of oxygen gas, does not burn.

8. A good many experiments on the combustion of this gas with oxygen by electricity give the following general result: 100 measures of the inflammable gas consume 205 measures of oxygen gas, and 101 measures of carbonic acid gas are formed. This result very nearly agrees with the experiments of Mr. Dalton, who found, as I have been informed by Dr. Henry, that 100 measures of this gas require for combustion 200 measures of oxygen gas, and form 100 measures of carbonic acid. The following table contains a few of the experiments which I made upon this gas. The others were precisely of the same kind, and coincided with these as nearly as possible.

The oxygen gas used contained 12 per cent. of azote. It had been prepared from hyper-oxymuriate of potash, and, when recent, contained only 1 per cent. of azote. It had been kept above a year in a crystal bottle well stopped;

 but

but had been often opened, and portions of it used during the interval. The bottle was now half-full of water. The purity of this gas was tried just at the time of making the following experiments, by exposing it to hydroguretted sulphuret of lime in the usual way. The nitrous gas employed contained, by the test of sulphate of iron, 14·5 per cent. of azote. The inflammable gas had been freed from carbonic acid, but it contained 13·5 per cent. of common air.

	Measures of inflammable gas.	Measures of oxygen.	Bulk of residue.	Bulk of do. when washed in lime-water.	Measures of nitrous gas added to do.	Bulk of residue.	Bulk of do. washed in sulphate of iron.
1	25	50	31	9	22	24	9
2	25	60	40	17	40	30	12
3	20	55	Does not burn.				
4	25	55	44	38	88	42	18
5	25	50	31	8·5	39	32	10
6	40	35	Does not burn.				
7	40	40	Burns.				

From this table, and our knowledge of the constituents of the gases used, it is easy to deduce the following:

	Gases examined.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure inflammable gas.	Pure oxygen.	Azote.	Total.			Azote.	Oxygen.
1	21·6	44·7	8·7	75	22	9	8·7	0·3
2	21·6	53·5	9·9	85	23	17	9·9	7·1
3	17·3	48·9	8·8	75				
4	2·16	49·1	9·3	80	6	38	9·3	29
5	21·6	44·7	8·7	75	22·5	8·5	8·7	0
6	34·6	31·9	8·5	75				
7	34·6	36·3	9·1	80				

Of these experiments the 1st, 2d, and 5th are the only ones from which the proportion of oxygen consumed, and of carbonic acid formed, can be deduced. They were repeated very often, in order to insure as much accuracy as possible. The following is the result which they give:

Inflammable gas consumed	Oxygen gas consumed.	Carbonic acid gas formed.	
21·6	44·7	22	
21·6	43·6	23	
21·6	44·7	22·5	
21·6	44·3	22·5	average.
100	205	104	aver. per cent.

9. From these experiments it is easy to deduce the composition of this inflammable gas. Its specific gravity being 0·555, 100 cubic inches of it, at the temperature of 60°, and when the barometer stands at 30 inches, will weigh 16·93 grains.

Of the 205 cubic inches of oxygen gas consumed, 104 go to the formation of carbonic acid gas; the remaining 101 cubic inches must have combined with hydrogen during the combustion, and formed water. Hydrogen, in the gaseous state, combines with exactly half its bulk of oxygen gas; therefore the hydrogen, which went to form water in the present case, must have been equivalent to 202 cubic inches.

If we suppose with Saussure, (and it is the supposition least favourable to our present purpose,) that carbonic acid gas contains 27·5 per cent. of carbon, in that case the carbon contained in 104 cubic inches of it must weigh 13·24 grains. The weight of 202 cubic inches of hydrogen gas is 5·17 grains. Thus we have ascertained, that 100 cubic inches of the inflammable gas from stagnant water contains of carbon .... 13·24

hydrogen .... 5·17

Total .... 18·41

But the weight of 100 cubic inches of the gas is only .... 16·93

So that the constituents found by analysis exceed the weight of the gas 1·48, or almost 1·5 grain.

This

This is a clear proof that the gas contains no oxygen. The carbon and hydrogen which we have found it to contain constitute the whole of its weight. The small excess would nearly vanish, if we were to suppose the specific gravity of the gas to be 0·600, as Dalton found it. It is obviously owing to errors in the analysis, which are unavoidable when minute quantities of gaseous bodies are measured.

10. We may conclude, then, that the gas from stagnant water is entitled to the name of carbureted hydrogen, and that it is a compound of very nearly,

Carbon	72
Hydrogen	28
	100

Mr. Dalton considers it as composed of an atom of carbon, and two atoms of hydrogen; and this very nearly agrees with the above analysis, if we suppose an atom of carbon to weigh 4·5, and an atom of hydrogen to weigh 1; for 72 is to 28 very nearly as 4·5 to 2.

There is another gas which is at present considered as composed of carbon and hydrogen. It was discovered by the associated Dutch chemists, and called by them olefiant gas, in consequence of the oily-looking substance into which it is converted when mixed with oxymuriatic acid gas. The experiments of Cruickshanks, Henry, Dalton, and Berthollet, all coincide to show that its only constituents are carbon and hydrogen. Mr. Murray, however, in his System of Chemistry, has expressed his suspicion that oxygen is also one of its constituents. He founds his conjecture on the alleged formation of carbonic acid gas when olefiant gas is passed through a red-rot tube. Even if carbonic acid were formed in this case, it would not prove the gas to contain oxygen, unless the experiment be made in such a manner as that all common air is completely excluded; and practical chemists are sufficiently aware of the difficulty of such an exclusion, when the experiment is made in the usual way. Carbonic acid gas never makes its appearance, if the whole common air be previously removed from the tube by means of a current of hydrogen, and if care be taken to separate all the oxygen with which the olefiant gas may be mixed, by means of nitrous gas. It is proper to know that liquid sulphuret of lime has the property of dissolving olefiant gas in considerable quantity. It cannot, therefore, be used to free the gas from oxygen.

Though the well-earned celebrity of the chemists who have

have analysed olefiant gas left little doubt that their analysis was accurate, I thought it worth while to repeat their experiments, in order to add my testimony to theirs.

1. One ounce measure of alcohol, of the specific gravity 0·826, and three ounce measures of sulphuric acid, of the specific gravity 1·860, were mixed together, and boiled in a small retort, almost to dryness; while the gaseous products were received in the usual manner over water. The gases obtained (not reckoning the common air of the retort, which amounted to about 42 cubic inches,) were 146·12 cubic inches of olefiant gas, and 53 cubic inches of carbonic acid gas. The proportion of carbonic acid at first was very small, but it increased as the process went on, and at the end amounted to about one-half of the olefiant gas. The charry matter which remained in the retort, after being well washed and dried on a sand-bath, weighed 68 grains.

2. A portion of this olefiant gas, deprived of its carbonic acid, but containing 16 per cent. of common air, was found of the specific gravity 0·9786, that of air being 1·000. Hence the specific gravity of pure olefiant gas is 0·9745. This is heavier than it was found by the Dutch chemists, who state its specific gravity at 0·909. As far as I know, the specific gravity has not been taken before in this country.

3. Being curious to know the nature of the supposed oil which is formed when olefiant gas is mixed with oxymuriatic acid gas, I filled a large bottle with olefiant gas, and passed a current of oxymuriatic acid gas into it: the wished-for substance soon collected at the bottom of the phial. It possessed the following properties.

(1.) It was a liquid of a greenish-white colour, with a slight smell of oxymuriatic acid, which it lost when allowed to stand a sufficient time exposed to the air. Its taste was sweet and cooling, and it made a strong, though not unpleasant, impression on the palate.

(2.) When dropped into water, it fell to the bottom, and looked like so much melted phosphorus. It continued in that state for some time if left at rest; but when the mixture was agitated, it dissolved in the water. The liquid continued colourless, acquired a sweet and cooling agreeable taste, and a slight aromatic odour. It did not affect vegetable blues, but precipitated copiously with nitrate of silver.

(3.) When dropped into alcohol, the liquid appeared milky at first, but almost immediately became transparent, a complete solution being effected.

(4.) Sul-

(4.) Sulphuric ether dissolved it with great rapidity, and the solution was colourless.

(5.) It did not dissolve in oil of turpentine, but continued in small globules, which attached themselves to the bottom of the vessel.

(6.) It dissolved immediately in nitric acid, without effervescence, or any apparent change in the liquid.

(7.) When dropped into sulphuric acid, a copious effervescence took place, the smell of oxymuriatic acid became evident, and the liquid remained clear.

(8.) When left in an open vessel, it evaporated completely, leaving only a green trace.

From these properties it is obvious that the liquid in question does not belong to the class of oils. It is a substance of a nature quite peculiar, and seems to consist of the two gases simply combined together. It has considerable resemblance, at least in taste, to the *pyro-acetic spirit* of Mr. Chenevix.

4. When olefiant gas is mixed with thrice its bulk of oxygen gas, it detonates very loudly, when an electric spark is passed through it, and burns with a strong white flame. According to Mr. Dalton, it consumes exactly thrice its bulk of oxygen gas, and forms twice its bulk of carbonic acid. My experiments, as will appear from the following table, very nearly coincide with his. The gas used contained 16 per cent. of common air, and the oxygen gas was mixed with 11 per cent. of azote.

	Measures of olefiant gas.	Measures of oxygen gas.	Residue after combustion.	Do. washed in lime-water.	Measures of nitrous gas added to do.	Residue.
1	20	59	45	10	42	44
2	20	60	45	9	23	27
3	20	57	42	8	23	27

From this table we easily deduce the following:

	Measures of Gases mixed.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure olefiant.	Pure oxygen.	Azote.	Total.			Azote.	Oxygen.
1	16·8	53·14	9·06	79	35	10	9·06	2·9
2	16·8	54·04	9·16	80	36	9	9·16	1·8
3	16·8	51·34	8·86	77	34	8	8·6	1·4

This table obviously furnishes us with the following results:

	Olefiant gas consumed.	Oxygen gas consumed.	Carbonic acid formed.	
1	16·8	50.24	35	
2	16 8	52·24	36	
3	16·8	49·94	34	
	16·8	50·8	35	Average.
	100	302	208	Aver. per cent.

From these experiments, it is easy to deduce the composition of olefiant gas: 100 cubic inches of it, at the temperature of 60°, and when the barometer stands at 30 inches, weigh 29·72 grains.

Of the 302 cubic inches of oxygen gas consumed, 208 went to the formation of carbonic acid. The remaining 94 cubic inches must have gone to the formation of water, and they must have combined with a quantity of hydrogen, which, if in the gaseous form, would have amounted to 188 cubic inches. Therefore 100 cubic inches of olefiant gas are composed of the carbon in 208 cubic inches of carbonic acid, and a quantity of hydrogen equivalent to 188 cubic inches.

	Grains.
Now, the carbon in 208 inches of carbonic acid weighs	26·98
108 inches of hydrogen gas weigh	4·80
Total	31·78
Weight of the olefiant gas	29·72
Surplus	2·06

Thus

Thus the weight of the constituents found by analysis exceeds that of the olefiant gas by about $\frac{1}{13}$th part;—a clear proof that olefiant gas contains no oxygen. The science of chemistry, in its present state, admits of no stronger proof than what we have now given.

It follows from the preceding analysis, that olefiant gas is composed of about

Carbon	85
Hydrogen	15
	100

Mr. Dalton considers it as composed of an atom of carbon and an atom of hydrogen. This comes tolerably near the preceding analysis, if we suppose the weight of an atom of carbon 4·5, and that of an atom of hydrogen 1. For 85 is to 15 very nearly as 4·5 is to 0·8.

6. There is a curious experiment, first made by Cruickshanks, and afterwards repeated by Berthollet, which I thought it worth while to verify. When olefiant gas is mixed with less than its bulk of oxygen, and the mixture is fired by electricity, a quantity of charcoal precipitates, and the bulk of the residue, after the detonation, is much greater than before. The following table exhibits the result of my experiments in this way:

	Measures of olefiant gas.	Measures of oxygen.	Residue after combustion.	Do. washed in lime-water.	Measures of nitrous gas added.	Residue.
1	39·5	28·5	100	94·5	37	121·5
2	40	30	110			
3	40	30	110			
4	40	29	110			

In each of these experiments, a quantity of charcoal was precipitated. It remained long suspended in the gaseous residue, quite dry, and made its escape into the air, if the detonating tube was turned up. I attempted to ascertain the weight of this charcoal, but was not successful. It was necessary, in order to collect it on the filter, to moisten it in the first place; and it was found impossible to dissipate the

the whole of the water, without altering the filter. Hence it was always a great deal too heavy, never weighing less than $\frac{1}{5}$th of a grain.

From the preceding table we easily deduce the following:

	Pure o-lefiant gas.	Pure oxygen gas.	Azotic gas.	Total.	Carbonic acid formed.	Residue.
1	33·2	26·64	8·16	68	5·5	94·5
2	33·6	27·98	8·42	70		
3	33·6	27·98	8·42	70		
4	33·6	27·08	8·32	69		

In the first experiment, 5·5 measures of oxygen gas went to the formation of carbonic acid, and 3·74 were found uncombined, after the explosion, by the test of nitrous gas. Hence it follows, that 17·4 measures of oxygen gas, combined with 33·2 measures of olefiant gas, deprived of a portion of its carbon, and formed a new inflammable gas, amounting to 82·7 measures in bulk, or almost double the bulk of the two gases that went to form it.

The new inflammable gas being examined by a new mixture with oxygen gas, and a new detonation, was found to consume 73 per cent. of oxygen gas, and to form 55 per cent. of carbonic acid.

Hence the 82·7 measures would have formed ..... 45·5
But 33·2 measures of olefiant gas would have formed 69·0

Difference .... 23·5

From this it would seem, that about $\frac{1}{4}$th of the carbon removed by the first explosion is converted into carbonic acid, while $\frac{3}{4}$ths precipitate in the state of a black powder.

33·2 cubic inches of olefiant gas weigh .... 9·87 grains.
The carbon in 23·5 cubic inches of carbonic acid weighs .... 3·05

Residue .... 6·82
17·4 cubic inches of oxygen weigh 5 31

Hence the weight of 82·7 cubic inches of the new gas cannot exceed .... 12·13

Hence

Hence 100 cubic inches of it would weigh 14·66 grains, and its specific gravity cannot exceed 0·4808.

This is on the most unfavourable supposition, that no water whatever is formed during the first combustion. If water be formed, it is obviously lighter than we have made it. It is clear, therefore, that this new-formed gas, to which the name of oxycarbureted hydrogen may with propriety be given, is quite different from carbonic oxide gas, the specific gravity of which is 0·956, or almost double of our new gas.

9·87 grains of olefiant gas are composed of 1·49 hydrogen.
8·38 carbon.

9·87

Hence our oxycarbureted hydrogen gas is composed of.................. 1·49 hydrogen.
5·33 carbon.
5·31 oxygen.

12·13

Or per cent. of	carbon	43·9
	oxygen	43·8
	hydrogen	12·3
		100·0

It is not improbable that this oxycarbureted hydrogen gas is composed of an atom of carbon, an atom of oxygen, and an atom of hydrogen. If that supposition be well-founded, the proportion of oxygen must exceed a little what we have obtained by our analysis. This would probably have been the case, if we had founded our analysis upon any of the succeeding experiments, rather than the first of the preceding table.

The preceding experiments, I flatter myself, entitle us to conclude, that *two* gaseous compounds of hydrogen and carbon exist. To the first we may give the usual name of carbureted hydrogen; to the second the name of super-carbureted hydrogen, as it contains very nearly twice as much carbon as the first gas does. There exists also a gaseous compound, consisting of oxygen, carbon, and hydrogen; but it differs in its properties from all other inflammable gases hitherto examined. The reason why the inflammable gases from vegetable and animal substances differ so much from each other is, that they usually hold an oil in solution, and are mixed with variable quantities of carbonic oxide gas.

XLVII. *On*

XLVII. *On a grand practical Improvement in the Harmony of Musical Instruments, by the Introduction of the Rev. Henry Liston's Patent* Enharmonic Organ; *with the Names of the 60 distinct Sounds which it produces in each Octave, from 20 Pipes, and their Intervals calculated. By Mr.* John Farey.

To Mr. Tilloch.

Sir, In your twenty-seventh volume, page 206, I endeavoured to call the attention of Lord Stanhope and other patrons of musical improvements, to the perfecting of an Organ capable of performing in *perfect tune*, without any tempered concords throughout the whole scale of 24 keys, on which the late Mr. Maxwell, of Broomholme, in Scotland, had written, and had applied the same successfully to the practice of the violin performer: and in your same volume, p. 315, I further said, "It follows, from the excellent writings of Mr. Maxwell, that, were *one* of the parts of a concert, the bass for instance, to be performed on a certain system of *fixed tones* (which it will be seen is quite consistent with perfect *chords*), that the number of notes would in this case not much exceed 60 within the octave, to effect *perfect harmony*, or the avoiding of all temperaments therein, in modulating through 24 keys."

In the course of much communication and correspondence with Musical Theorists since the above period, I have been surprised to find them, either insensible or inattentive to this grand desideratum in music, and several of them disposed even to ridicule the very attempt.

It gives me great pleasure therefore to be able to state, that the above is now no longer a matter of doubtful speculation; but that myself and several others have heard, and doubtless hundreds of others will shortly hear, an Organ thus perfected, in Scotland, by the Rev. Henry Liston, and now exhibiting at Flight and Robson's, organ-builders, in St. Martin's Lane; the exquisite effect of which, particularly in accompanying vocal music, far exceeds all that Maxwell or myself had written or perhaps conceived of the harmony of such an instrument. The patentee is now receiving subscriptions for a work, which will satisfactorily explain, 1st, The mathematical theory, the construction of the instrument, and the application of the theory to tuning it; and, 2dly, An application of his theory to the various species of musical composition. The first part of this

 gen-

gentleman's manuscript work I have perused, with much pleasure and satisfaction, and conceive that I shall be doing an acceptable service to many of your readers, in presenting a Table of the 60 notes, (for he omits ♭F, and its grave and acute), calculated according to the following tuning process, in a notation of my own, which I some time ago adopted, as the simplest for these kind of musical calculations, and therefore called it the "Elements of perfect Tune," wherein no interval smaller than the *Schisma* (Σ) occurs, and wherein the *minor comma*, ($\mathfrak{C}$, or $10\Sigma + m$ of my other notation) is another interval frequently occurring, particularly in conjunction with the schisma, to make a major comma; and the *semitone subminimis* of Overend ($\mathfrak{f}$*, or $25\Sigma + f + 2m$) which is the interval generally occurring between the groups of notes belonging to the different finger-keys: for it will be observed, that in this or even still more extended perfect scales, the notes are at very unequal distances apart, though their relations may almost all be expressed by the above notation, and all calculations thereon performed thereby, without negative signs, or any unnecessary complication of numbers.

The values of the intervals are contained in two columns in the middle of the table, the left hand ones are those of the 20 principal notes in each octave, whose letters, names, and marks succeed in proceeding outwards: the second column contains the 40 sounds, produced by raising and by depressing each note in the first, a major comma, or 0 1 1, in this notation, and the letters and marks (′ and ‵ to express this raising and depressing by a comma, called acute and grave) of these and the names, succeed each other outwards. My nomenclature or names of several of these intervals, above C, will be found to differ somewhat from those of the author, who considers the terms *in use* among practical musicians, as to be preferred for his purposes, though somewhat loose and indefinite, to others free of this defect, which they might have to learn. The major and minor intervals are fixed, by the nature of the scale, and to these all my names refer, by schisma 0 0 1 (Σ); minor comma 0 1 0 ($10\Sigma + m$); major comma 0 1 1 ($11\Sigma + m$); or superfluous 1 1 1 ($36\Sigma + f + 3m$) the minor semitone; all by fixed and invariable appellations.

A principal improvement by Mr. Liston consists in his mode of obtaining the 20 notes of the scale for his present organ,

* We have here, for the convenience of printing, substituted the old English f, instead of the script long *s*, used in the copy and in the engraved Table Plate V, in our xxviiith volume.—EDIT.

organ, and by which he has been able to avoid that impracticable divergency of tune which Mr. Maxwell's scale for the organ contains. It is thus that Mr. L. tunes, upwards:

C $^{\text{v}}$ G $^{\text{v}}$ D: then C $^{\text{III}}$ E $\underset{\text{4th}}{\frown}$ A B $^{\text{v}}$ *F $^{\text{v}}$ *C: then E $^{\text{III}}$ *G $^{\text{v}}$ *D $^{\text{v}}$

*A $^{\text{v}}$ *E: and *G $^{\text{III}}$ *B. And downwards,

C $^{\text{v}}$ F $^{\text{v}}$ ♭B: and then C $^{\text{III}}$ ♭A $\underset{\text{4th}}{\frown}$ ♭E $^{\text{v}}$ ♭D $^{\text{v}}$ ♭G $^{\text{v}}$ ♭C.

Instead of which, Mr. Maxwell proposed a series of diatonic scales, proceeding always by perfect major fifths, and therefore his scale, though containing 44 sounds in the octave, could not furnish a great many intervals wanted by the musician, or admit of the modulations in daily use. Mr. L. on the contrary, by proceeding as above, and extending the number of notes to 24, or as many more as may be desired, produces his notes all in true musical relation to one fundamental bass note C; and as within such a scale, there are many intervals false by a comma, he enables the performer in every instance to correct the error, by means of his pedals: and I am assured, that the use of these pedals has been found so easy and simple, as to occasion little difficulty even at first trial, and that a day or two has always sufficed, to make performers of ordinary capacity, perfectly acquainted with them.

It is very easy to apply the above tuning scale to my calculations of the notes: thus the V$^{\text{th}}$ G is fixed by the nature of this notation at 7 17 13, this added to itself, gives 14 34 26, for *d*, in the octave above, therefore deduct the octave 12 29 22 (which the nature of this notation has fixed), and we have D = 2 5 4: E the III$^{\text{d}}$ is fixed at 4 9 7 (by the nature of the scale), to which we add a 4$^{\text{th}}$, 5 12 9, and we have A = 9 21 16; and so for any other notes in this table, or for more extended scales wherein ♭F and the necessary double flats and sharps are introduced, as can with facility be done by the inventor, on organs for which he may be honoured with orders. I cannot conclude without earnestly calling the attention of all lovers of good harmony to this improvement, and wishing every success to the very able and ingenious inventor.

I remain, sir,

Your obedient servant,

JOHN FAREY.

12, Upper Crown street, Westminster,
April 12, 1811.

 The

The Scale of 60 Notes, on the Rev. Henry Liston's *Patent Enharmonic Organ (now exhibiting at* Flight *and* Robson's, *in St. Martin's Lane), calculated in Elements of perfect Tune: with the Names of the Intervals, by* John Farey.——*April 8, 1811.*

			f.	E.	Σ.	f.	E.	Σ.		
VIII.	*Major* Eighth or *Octave*	c	12	29	22					
						12	28	22	*B′	Minor-comma-defective major *Eighth.*
						12	28	21	c‵	Comma-deficient major *Eighth.*
	Superfluous major *Seventh*	*B	12	27	21					
						12	26	20	*B‵	Double-superfluous minor *Seventh.*
						11	28	21	b c′	Comma redundant minor *Eighth.*
						11	27	21	B′	Comma-redundant major *Seventh.*
8th.	*Minor Eighth*	bc	11	27	20					
VII.	*Major* Seventh	B	11	26	20					
						11	26	19	b c‵	Comma-deficient minor *Eighth.*
						11	25	19	B‵	Comma-deficient major *Seventh.*
						10	25	19	bB′	Comma-redundant minor *Seventh.*
						10	24	19	*A′	Schisma-excessive minor *Seventh.*
7th.	*Minor* Seventh	bB	10	24	18					
	Minor-comma-defective minor *Seventh*	*A	10	23	18					
						10	23	17	bB‵	Comma-deficient minor *Seventh.*
						10	22	17	*A‵	Superfluous major *Sixth.*
						9	22	17	A′	Comma-redundant major *Sixth.*
VI.	*Major* Sixth	A	9	21	16					
						9	20	15	A‵	Comma-deficient major *Sixth.*
						8	21	16	bA′	Comma redundant minor *Sixth.*
6th.	*Minor* Sixth	bA	8	20	15					
						8	19	15	*G′	Minor-comma-defective minor *Sixth.*
						8	19	14	bA‵	Comma deficient minor *Sixth.*
	Superfluous major *Fifth*	*G	8	18	14					
						8	17	13	*G‵	Double-superfluous minor *Fifth.*
						7	18	14	G′	Comma-redundant major *Fifth.*
V.	*Major* Fifth	G	7	17	13					

			l.	Є.	Σ.	l.	Є.	Σ.	
						7	16	12	G' Comma-deficient major *Fifth.*
						6	16	12	bG' Comma-redundant minor *Fifth.*
						6	15	12	✱F' Comma-redundant major *Fourth.*
5th.	*Minor* Fifth - - (or Semidiapente) -	bG	6	15	11				
IV.	*Major* Fourth - - (or Tritone) -	✱F	6	14	11				
						6	14	10	bG' Comma-deficient minor *Fifth.*
						6	13	10	✱F' Comma-deficient major *Fourth.*
						5	13	10	F' Comma-redundant minor *Fourth.*
						5	12	10	✱E' Schisma-excessive minor *Fourth.*
4th.	*Minor* Fourth - - - - - -	F	5	12	9				
	Minor-comma-defective minor *Fourth* -	✱E	5	11	9				
						5	11	8	F' Comma-deficient minor *Fourth.*
						5	10	8	✱E' Superfluous major *Third.*
						4	10	8	E' Comma-redundant major *Third* - - (or Ditone).
III.	*Major* Third - - - - - -	E	4	9	7				
						4	8	6	E' Comma-deficient major *Third.*
						3	9	7	bE' Comma redundant minor *Third.*
3d.	*Minor* Third - - - - - -	bE	3	8	6				
						3	7	6	✱D' Minor-comma-defective minor *Third.*
						3	7	5	bE' Comma-deficient minor *Third.*
	Superfluous major *Second* - - -	✱D	3	6	5				
						3	5	4	✱D' Double superfluous minor *Second.*
						2	6	5	D' Comma-redundant major *Second*, or *Tone* maximum.
II.	*Major* Second, - - or *Tone* major (T) - (t)	D	2	5	4				
						2	4	3	D' Comma-deficient major *Second*, - or *Tone* minor.
						1	4	3	bD' Comma redund. minor *Second*, or *Semitone* maximum.
						1	3	3	✱C' Schisma-excessive minor *Second*, - - or *Apotome.*
2d.	*Minor* Second, - - or *Semitone* major (S) -	bD	1	3	2				
	Minor-comma-defective minor *Second*, or *Semitone* medius - - -	✱C	1	2	2				
						1	2	1	bD' Comma-deficient minor *Second*, - - or *Limma.*
						1	1	1	✱C' Superfluous *Unison*, - - - or *Semitone* minor.
							1	1	C' Comma-redundant *Unison*, - - or Major *Comma.*

P. S.—Some may wish to know, that
$\mathfrak{f}$ the *semitone* subminimis $= 2t - T - S = 2{\cdot}514561 \times \epsilon$
$= 25{\cdot}165387 \times \Sigma$.
ϵ the minor *comma* .... $= 2S - T = 10{\cdot}0078624 \times \Sigma$
$= {\cdot}3976837 \times \mathfrak{f}$: and,
Σ the *schisma* .... $= 2T - t - 2S = {\cdot}0999204 \times \epsilon$
$= {\cdot}03973712 \times \mathfrak{f}$.

The schisma-excessive minor *Fourth* $5\mathfrak{f} + 12\epsilon + 10\Sigma$, belongs to the scale of my Equal Temperament (mentioned in your 28th volume, p. 65; 35th volume, p. 452; and 36th volume, p. 48), and exceeds the true Isotonic Fourth or $\frac{5}{12}$ths of the octave, by only about the $\frac{1}{10801}$st part of a major comma: it is equal 3V + III − 4 4ths, and is tuned, or may be proved on this organ, by trying C, G D A′ and ∗C upwards and thence ∗G′, ∗D′, ∗A′ and ∗E′ downwards, when C will be found to beat with this last note, 1·0836 times per second, if C of the tenor cliff line on the diapason stop vibrates 240 times per second, according to the true concert pitch.—J. F.

XLVIII. *Experiments on Allanite, a new Mineral from Greenland.* By THOMAS THOMSON, *M.D.F.R.S. Fellow of the Imperial Chirurgo-Medical Academy of Petersburgh**.

ABOUT three years ago, a Danish vessel † was brought into Leith as a prize. Among other articles, she contained a small collection of minerals, which were purchased by Thomas Allan, Esq. and Colonel Imrie. The country from which these minerals had been brought was not known for certain; but as the collection abounded in cryolite, it was conjectured, with very considerable probability, that they had been collected in Greenland.

Among the remarkable minerals in this collection, there was one, which, from its correspondence with gadolinite, as described in the different mineralogical works, particularly attracted the attention of Mr. Allan, who was confirmed in the idea of its being a variety of that mineral by the opinion of Count Bournon, added to some experiments made by Dr. Wollaston; and he was thus induced to give the description which has since been published in a preceding part of the present volume.

* From the Transactions of the Royal Society of Edinburgh.

† Der Fruhling, Captain Jacob Ketelson, captured on her passage from Iceland to Copenhagen.

About

About a year ago, Mr. Allan, who has greatly distinguished himself by his ardent zeal for the progress of mineralogy in all its branches, favoured me with some specimens of this curious mineral, and requested me to examine its composition,—a request which I agreed to with pleasure, because I expected to obtain from it a quantity of *yttria*, an earth which I had been long anxious to examine, but had not been able to procure a sufficient quantity of the Swedish gadolinite for my purpose. The object of this paper is to communicate the result of my experiments to the Royal Society,—experiments which cannot appear with such propriety any where as in their Transactions, as they already contain a paper by Mr. Allan on the mineral in question.

I. *Description.*

I am fortunately enabled to give a fuller and more accurate description of this mineral than that which formerly appeared, Mr. Allan having, since that time, discovered an additional quantity of it, among which he not only found fresher and better characterized fragments, but also some entire crystals. In its composition it approaches most nearly to cerite, but it differs from it so much in its external characters, that it must be considered as a distinct species. I have therefore taken the liberty to give it the name of *Allanite*, in honour of Mr. Allan, to whom we are in reality indebted for the discovery of its peculiar nature.

Allanite occurs massive and disseminated, in irregular masses, mixed with black mica and feldspar; also crystallized: the varieties observed are,

1. A four sided oblique prism, measuring 117° and 63°.
2. A six-sided prism, acuminated with pyramids of four sides, set on the two adjoining opposite planes. These last are so minute as to be incapable of measurement. But, as nearly as the eye can determine, the form resembles Fig. 3, Plate VI; the prism of which has two right angles, and four measuring 135°.
3. A flat prism, with the acute angle of 63° replaced by one plane, and terminated by an acumination, having three principal facettes set on the larger lateral planes, with which the centre one measures 125° and 55°. Of this specimen, an engraving is given in the Plate, Fig. 4.

Specific gravity, according to my experiments, 3·533. The specimen appears to be nearly, though not absolutely, pure. This substance, however, is so very much mixed with mica, that no reliance can be placed on any of the trials which have been made. Count Bournon, surprised

at the low specific gravity noted by Mr. Allan, which was 3·480, broke down one of the specimens which had been sent him, in order to procure the substance in the purest state possible, and the result of four experiments was as follows: 4·001, 3·797, 3·654, 3·119.——In a subsequent experiment of Mr. Allan's, he found it 3·665. From these it appears that the substance is not in a pure state. Its colour is so entirely the same with the mica, with which it is accompanied, that it is only by mechanical attrition that they can be separated.

Colour, brownish-black.

External lustre, dull; internal, shining and resinous, slightly inclining to metallic.

Fracture, small conchoidal.

Fragments, indeterminate, sharp-edged.

Opake.

Semi-hard in a high degree. Does not scratch quartz nor feldspar, but scratches hornblende and crown-glass.

Brittle.

Easily frangible.

Powder, dark greenish-gray.

Before the blow-pipe it froths, and melts imperfectly into a brown scoria.

Gelatinizes in nitric acid. In a strong red heat it loses 3·98 per cent. of its weight.

II. *Experiments to ascertain its Composition.*

My first experiments were made, on the supposition that the mineral was a variety of gadolinite, and were pretty much in the style of those previously made on that substance by Ekeberg, Klaproth, and Vauquelin.

1. 100 grains of the mineral, previously reduced to a fine powder in an agate mortar, were digested repeatedly on a sand-bath in muriatic acid, till the liquid ceased to have any action on it. The undissolved residue was silica, mixed with some fragments of mica. When heated to redness, it weighed 33·4 grains.

2. The muriatic acid solution was evaporated almost to dryness, to get rid of the excess of acid, dissolved in a large quantity of water, mixed with a considerable excess of carbonate of ammonia, and boiled for a few minutes. By this treatment, the whole contents of the mineral were precipitated in the state of a yellowish powder, which was separated by the filter, and boiled, while still moist, in potash-ley. A small portion of it only was dissolved. The potash-ley was separated from the undissolved portion by the

the filter, and mixed with a solution of sal ammoniac, by means of which a white powder precipitated from it. This white matter being heated to redness, weighed 7·9 grains. It was digested in sulphuric acid, but 3·76 grains refused to dissolve. This portion possessed the properties of silica. The dissolved portion being mixed with a few drops of sulphate of potash, shot into crystals of alum. It was therefore alumina, and amounted to 4·14 grains.

3. The yellow matter which refused to dissolve in the potash-ley was mixed with nitric acid. An effervescence took place, but the liquid remained muddy, till it was exposed to heat, when a clear reddish-brown solution was effected. This solution was evaporated to dryness, and kept for a few minutes in the temperature of about 400°, to peroxidize the iron, and render it insoluble. A sufficient quantity of water was then poured on it, and digested on it for half-an-hour, on the sand-bath. The whole was then thrown upon a filter. The dark red matter which remained on the filter was drenched in oil, and heated to redness, in a covered crucible. It was then black, and attracted by the magnet; but had not exactly the appearance of oxide of iron. It weighed 42·4 grains.

4. The liquid which passed through the filter had not the sweet taste which I expected, but a slightly bitter one, similar to a weak solution of nitrate of lime. Hence it was clear that no yttria was present, as there ought to have been, had the mineral contained that earth. This liquid being mixed with carbonate of ammonia, a white powder precipitated, which, after being dried in a red heat, weighed 17 grains. It dissolved in acids with effervescence: the solution was precipitated white by oxalate of ammonia, but not by pure ammonia. When dissolved in sulphuric acid, and evaporated to dryness, a light white matter remained, tasteless, and hardly soluble in water. These properties indicate carbonate of lime. Now, 17 grains of carbonate of lime are equivalent to about 9·23 grains of lime.

5. From the preceding analysis, supposing it accurate, it followed that the mineral was composed of

Silica	37·16
Lime	9·23
Alumina	4·14
Oxide of iron	42·40
Volatile matter	3·98
	96·91
Loss	3·09
	100·00

But

But the appearance of the supposed oxide of iron induced me to suspect that it did not consist wholly of that metal. I thought it even conceivable, that the yttria which the mineral contained might have been rendered insoluble by the application of too much heat, and might have been concealed by the iron with which it was mixed. A number of experiments, which it is needless to specify, soon convinced me that, besides iron, there was likewise another substance present, which possessed properties different from any that I had been in the habit of examining. It possessed one property at least in common with yttria; its solution in acids had a sweet taste; but few of its other properties had any resemblance to those which the chemists to whom we are indebted for our knowledge of yttria have particularized. But as I had never myself made any experiments on yttria, I was rather at a loss what conclusion to draw. From this uncertainty I was relieved by Mr. Allan, who had the goodness to give me a small fragment of gadolinite, which had been received directly from Mr. Ekeberg. From this I extracted about 10 grains of yttria; and upon comparing its properties with those of the substance in question, I found them quite different. Convinced by these experiments that the mineral contained no yttria, but that one of its constituents was a substance with which I was still unacquainted, I had recourse to the following mode of analysis, in order to obtain this substance in a pure state.

III. *Analysis of Allanite.*

1. 100 grains of the mineral, previously reduced to a fine powder, were digested in hot nitric acid till nothing more could be dissolved. The undissolved residue, which was silica, mixed with some scales of mica, weighed, after being heated to redness, 35·4 grains.

2. The nitric acid solution was transparent, and of a light-brown colour. When strongly concentrated by evaporation, to get rid of the excess of acid, and set aside in an open capsule, it concreted into a whitish solid matter, consisting chiefly of soft crystals, nearly colourless, having only a slight tinge of yellow. These crystals being left exposed to the air became gradually moist, but did not speedily deliquesce. The whole was therefore dissolved in water, and the excess of acid, which was still present, carefully neutralized with ammonia. By this treatment, the solution acquired a much deeper brown colour; but still continued transparent. Succinate of ammonia was then dropped

dropped in with caution. A copious reddish-brown precipitate fell, which being washed, dried, and heated to redness in a covered crucible, weighed 25·4 grains. It possessed all the characters of black oxide of iron. For it was attracted by the magnet, completely soluble in muriatic acid, and the solution was not precipitated by oxalate of ammonia.

3. The liquid being still of a brown colour, I conceived it not to be completely free from iron. On this account, an additional quantity of succinate of ammonia was added. A new precipitate fell; but instead of the dark reddish-brown colour which characterizes succinate of iron, it had a beautiful flesh-red colour, which it retained after being dried in the open air. When heated to redness in a covered crucible, it became black, and had some resemblance to gunpowder. It weighed 7·2 grains.

4. This substance attracted my peculiar attention, in consequence of its appearance. I found it to possess the following characters:

a. It was tasteless, and not in the least attracted by the magnet, except a few atoms, which were easily separated from the rest.

b. It was insoluble in water, and not sensibly acted on when boiled in sulphuric, nitric, muriatic, or nitro-muriatic acid.

c. Before the blow-pipe it melted with borax and microcosmic salt, and formed with both a colourless bead. With carbonate of soda it formed a dark-red opake bead.

d. When heated to redness with potash, and digested in water, snuff-coloured flocks remained undissolved, which gradually subsided to the bottom. The liquid being separated, and examined, was found to contain nothing but potash. When muriatic acid was poured upon the snuff-coloured flocks, a slight effervescence took place, and when heat was applied, the whole dissolved. The solution was transparent, and of a yellow colour, with a slight tint of green. When evaporated to dryness, to get rid of the excess of acid, a beautiful yellow matter gradually separated. Water boiled upon this matter dissolved the whole. The taste of the solution was astringent, with a slight metallic flavour, by no means unpleasant, and no sweetness was perceptible.

e. A portion of the black powder being exposed to a red heat for an hour, in an open crucible, became reddish-brown, and lost somewhat of its weight. In this altered state, it was soluble by means of heat, though with difficulty,

culty, both in nitric and sulphuric acids. The solutions had a reddish-brown colour, a slight metallic astringent taste, but no sweetness.

f. The solution of this matter in nitric and muriatic acid, when examined by re-agents, exhibited the following phænomena:

(1.) With prussiate of potash, it threw down a white precipitate in flocks. It soon subsided; readily dissolved in nitric acid; the solution was given.

(2.) Prussiate of mercury. A light yellow precipitate, soluble in nitric acid.

(3.) Infusion of nut galls. No change.

(4.) Gallic acid. No change.

(5.) Oxalate of ammonia. No change.

(6.) Tartrate of potash. No change.

(7.) Phosphate of soda. No change.

(8.) Hydro-sulphuret of ammonia. Copious black flocks. Liquor remains transparent.

(9.) Arseniate of potash. A white precipitate.

(10.) Potash............... }
(11.) Carbonate of soda. .... } Copious yellow-coloured flocks; readily dissolved in nitric acid.
(12.) Carbonate of ammonia. }

(13.) Succinate of ammonia. A white precipitate.

(14.) Benzoate of potash. A white precipitate.

(15.) A plate of zinc being put into the solution in muriatic acid, became black, and threw down a black powder, which was insoluble in sulphuric, nitric, muriatic, nitro-muriatic, acetic, and phosphoric acids, in every temperature, whether these acids were concentrated or diluted.

(16.) A plate of tin put into the nitric solution, occasioned no change.

(17.) A portion being inclosed in a charcoal crucible, and exposed for an hour to the heat of a forge, was not reduced to a metallic button, nor could any trace of it be detected when the crucible was examined.

These properties were all that the small quantity of the matter in my possession enabled me to ascertain. They unequivocally point out a metallic oxide. Upon comparing them with the properties of all the metallic oxides known, none will be found with which this matter exactly agrees. Cerium is the metal the oxides of which approach the nearest. The colour is nearly the same, and both are precipitated white by prussiate of potash, succinate of ammonia, and benzoate of potash. But, in other respects, the two substances differ entirely. Oxide of cerium is precipitated

cipitated white by oxalate of ammonia and tartrate of potash; our oxide is not precipitated at all: oxide of cerium is precipitated white by hydro-sulphuret of ammonia; while our oxide is precipitated black: oxide of cerium is not precipitated by zinc, while our oxide is thrown down black. There are other differences between the two, but those which I have just mentioned are the most striking.

These properties induced me to consider the substance which I had obtained from the Greenland mineral as the oxide of a metal hitherto unknown; and I proposed to distinguish it by the name of *junonium*.

In the experiments above detailed, I had expended almost all the oxide of *junonium* which I had in my possession, taking it for granted that I could easily procure more of it from the Greenland mineral. But, soon after, I was informed by Dr. Wollaston, to whom I had sent a specimen of the mineral, that he had not been able to obtain any of my supposed junonium in his trials. This induced me to repeat the analysis no less than three times, and in neither case was I able to procure any more of the substance which I have described above. Thus it has been out of my power to verify the preceding details, and to put the existence of a new metal in the mineral beyond doubt. At the same time, I may be allowed to say, that the above experiments were made with every possible attention on my part, and most of them were repeated at least a dozen times. I have no doubt myself of their accuracy; but think that the existence of a new metal can hardly be admitted, without stronger proofs than the solitary analysis which I have performed.

5. The liquid, thus freed from iron and junonium, was super-saturated with pure ammonia. A grayish-white gelatinous matter precipitated. It was separated by the filter, and became gradually darker-coloured when drying. This matter, after being exposed to a red heat, weighed about 38 grains. When boiled in potash-ley, 4·1 grains were dissolved, of a substance which, separated in the usual way, exhibited the properties of alumina.

6. The remaining 33·9 grains were again dissolved in muriatic acid, and precipitated by pure ammonia. The precipitate was separated by the filter, and allowed to dry spontaneously in the open air. It assumed an appearance very much resembling gum-arabic, being semi-transparent, and of a brown colour. When dried upon the sand-bath, it became very dark-brown, broke with a vitreous fracture, and still retained a small degree of transparency. It was tasteless,

felt

felt gritty between the teeth, and was easily reduced to powder. It effervesced in sulphuric, nitric, muriatic, and acetic acids, and a solution of it was effected in each by means of heat, though not without considerable difficulty. The solutions had an austere and slightly sweetish taste. When examined by re-agents, they exhibited the following properties:

(1.) Prussiate of potash. A white precipitate.
(2.) Oxalate of ammonia. A white precipitate.
(3.) Tartrate of potash. A white precipitate.
(4.) Hydro-sulphuret of potash. A white precipitate.
(5.) Phosphate of soda. A white precipitate.
(6.) Arseniate of potash. A white precipitate.
(7.) Potash and its carbonate. A white precipitate.
(8.) Carbonate of ammonia. A white precipitate.
(9.) Ammonia. A white gelatinous precipitate.
(10.) A plate of zinc. No change.

These properties indicated oxide of cerium. I was therefore disposed to consider the substance which I had obtained as oxide of cerium. But on perusing the accounts of that substance, given by the celebrated chemists to whose labours we are indebted for our knowledge of it, there were several circumstances of ambiguity which occurred. My powder was dissolved in acids with much greater difficulty than appeared to be the case with oxide of cerium. The colour of my oxide, when obtained from oxalate by exposing it to a red heat, was much lighter, and more inclined to yellow, than the oxide of cerium.

In this uncertainty, Dr. Wollaston, to whom I communicated my difficulties, offered to send me down a specimen of the mineral called *cerite*, that I might extract from it real oxide of cerium, and compare my oxide with it. This offer I thankfully accepted*; and upon comparing the properties of my oxide with those of oxide of cerium extracted from *cerite*, I was fully satisfied that they were identical. The more difficult solubility of mine was owing to the method I had employed to procure it, and to

* The specimen of cerite which I analysed, was so much mixed with actinolite, that the statement of the results which I obtained cannot be of much importance. The specific gravity of the specimen was 4·149. I found it composed as follows:

A white powder, left by muriatic acid, and presumed to be silica	47·3
Red oxide of cerium - - - - - - - -	44·
Iron - - - - - - - - - - -	4·
Volatile matter - - - - - - - - -	3·
Loss - - - - - - - - - - -	1·7
	100·0

the

the strong heat to which I had subjected it: whereas the oxide of cerium from cerite had been examined in the state of carbonate.

7. In the many experiments made upon this powder, and upon oxide of cerium from cerite, I repeated every thing that had been established by Berzelius and Hisinger, Klaproth and Vauquelin, and had an opportunity of observing many particulars which they have not noticed. It may be worth while, therefore, without repeating the details of these chemists, to mention a few circumstances, which will be found useful in examining this hitherto scarce oxide.

a. The precipitate occasioned by oxalate of ammonia is at first in white flocks, not unlike that of muriate of silver, but it soon assumes a pulverulent form. It dissolves readily in nitric acid, without the assistance of heat. The same remark applies to the precipitate thrown down by tartrate of potash. But tartrate of cerium is much more soluble in acids than the oxalate.

b. The solution of cerium in acetic acid is precipitated gray by infusion of nut-galls. Cerium is precipitated likewise by the same re-agent from other acids, provided the solution contain no excess of acid. This fact was first observed by Dr. Wollaston, who communicated it to me last summer. I immediately repeated his experiments with success.

c. Cerium is not precipitated from its solution in acids by a plate of zinc. In some cases, indeed, I have obtained a yellowish-red powder, which was thrown down very slowly. But it proved, on examination, to consist almost entirely of red oxide of iron, and of course only appeared when the solution of cerium was contaminated with iron.

d. The solutions of cerium in acids have an astringent taste, with a perceptible sweetness, which however is different from the sweetness which some of the solutions of iron in acids possess.

e. The muriate and sulphate of cerium readily crystallize; but I could not succeed in obtaining crystals of nitrate of cerium.

f. The best way of obtaining pure oxide of cerium is to precipitate the solution by oxalate of ammonia, wash the precipitate well, and expose it to a red heat. The powder obtained by this process is always red; but it varies very much in its shade, and its beauty, according to circumstances. This powder always contains carbonic acid.

g. I consider the following as the essential characters of

cerium.

cerium. The solution has a sweet astringent taste: it is precipitated white by prussiate of potash, oxalate of ammonia, tartrate of potash, carbonate of potash, carbonate of ammonia, succinate of ammonia, benzoate of potash, and hydro-sulphuret of ammonia: the precipitates are redissolved by nitric or muriatic acids: ammonia throws it down in gelatinous flocks: zinc does not precipitate it at all.

h. The white oxide of cerium, mentioned by Hisinger and Berzelius, and described by Vauquelin, did not present itself to me in any of my experiments; unless the white flocks precipitated by ammonia from the original solution be considered as white oxide. They became brown on drying, and when heated to redness were certainly converted into red oxide.

As cerium as well as iron is precipitated by succinate of ammonia, the preceding method of separating the two from each other was not unexceptionable. Accordingly, in some subsequent analyses, I separated the cerium by means of oxalate of ammonia, before I precipitated the iron. I found that the proportions obtained by the analysis above described, were so near accuracy that no material alteration is necessary.

8. The liquid, thus freed from iron, alumina, and cerium, was mixed with carbonate of soda. It precipitated a quantity of carbonate of lime, which amounted, as before, to about 17 grains, indicating 9·2 grains of lime.

From the preceding analysis, which was repeated no less than three times, a different method being employed in each, the constituents of allanite are as follows:

Silica	35·4
Lime	9·2
Alumina	4·1
Oxide of iron	25·4
Oxide of cerium	33·9
Volatile matter	4·
	112·0

I omit the 7 grains of junonium, because I only detected it in one specimen of allanite. The excess of weight in the preceding numbers is to be ascribed chiefly to the carbonic acid combined with the oxide of cerium, from which it was not completely freed by a red heat. I have reason to believe, too, that the proportion of iron is not quite so much as 25·5 grains. For, in another analysis, I obtained only 18 grains, and in a third 20 grains. Some of the cerium was perhaps precipitated along with it in the preceding analysis, and thus its weight was apparently increased.

XLIX. *On*

XLIX. *On the Sarcocele of Egypt**.

THE word *Sarcoceles*† is derived from the Greek (σαρξ κηλη). Fabricius ab Aquapendente, Fabricius Hildanus, Lanfranc, Fallopius, André de Lacroix, and others, have described this disease under the name of *caro adnata ad testem*. Since these authors, whose observations seem to relate to the sarcocele of warm climates, modern surgeons have confounded it with diseases of the testis, such as swelling, inflammation, scirrhus, hydrocele and hydrosarcocele. The etymology of the word sarcocele, and the signification which ancient authors seem to attach to it, proves that this denomination belongs exclusively to that disease which distends beyond measure the teguments external to the testicle, particularly the scrotum and dartos; and gives to these parts an extraordinary volume or size. The great number of individuals I have seen attacked with this complaint in Egypt confirms me in this opinion, and induces me to trace the causes, symptoms, progress, and effects, and to point out the means of relief, which are in the power of art.

My researches into the nature of sarcocele induce me to believe that it is confined to warm climates, at least, that it is very rarely met with in cold regions, for the great number of examples which we meet with in Europe are the produce of Asia or Africa. The scrotal tumour of the minister Charles Delacroix is perhaps a single instance of sarcocele well marked which has taken place in our climates, and the volume of this tumour was small, in comparison of the cases of sarcocele related in the German Ephemeris for 1692, in Dionis's chirurgical works, the *Bibliothèque de Médecine*, tome ix. and of those which I have seen with astonishment in Egypt, some of which weighed at least fifty pounds. I

* From Mons. *Larrey's Rélation Chirurgicale de l'Armée de l'Orient.*

† Under the term *sarcocele* a disease is here described differing essentially from the affection which in this country has been designated by that appellation. The sarcocele of Europe is a disease of the body of the testicle, liable to become seriously active from external accident, from irritating applications, and from incisions made into it when mistaken for hydrocele. But we are assured in this memoir, that the sarcocele of Egypt and other hot climates, is a carneous mass enormous in size, possessing little sensibility, having no other connexion with the testicle than being in its neighbourhood; bearing the potential and even the actual cautery, and suffering setons to be passed through it, with impunity; and occurring also in the female. The sarcocele of Europe is an organic disease of the testes; the sarcocele of Egypt is a disease of the scrotum and capsulæ of the testes in the male, and of the common teguments and cellular substance of the labia pudendi of the female, connected with elephantiasis, and being, possibly, one form of that disease.

shall relate some of those cases which are most worthy of remark. By sarcocele, properly so called, I mean that tumour which is formed in the scrotum, and is a fleshy mass, broad at its inferior extremity, and attached to the pubis by a peduncle or neck more or less extensive.

Externally it is rugous and irregular, separated by furrows or cavities, corresponding with the mucous cryptæ, or the roots of the hairs; yellow scaly incrustations are usually found on its surface, more particularly if the sarcocele be of long duration. When these crusts fall off, there are underneath several small ulcers, which discharge an ichorous fluid. The tumour is indolent and harder at some parts than at others; it may be felt and pressed upon in any direction without exciting the smallest pain.

The patient is no otherwise incommoded by it than by its weight, and the impediment it proves to walking, which compels him to have it suspended. The urine flows over the tumour without excoriating it.

In the great number of sarcoceles which I have seen, I have remarked that the testicles and spermatic chord were in a sound and natural state, and situated on the sides of the tumour; but the vessels of the testis were usually augmented in size (varicose). If the testicle should participate in the disease, it will be accompanied by the symptoms proper to such affection. It does not, however, seem to me that the testis is capable of so great an augmentation of size with whatever disease it may be affected; for the patient would sink if the morbid alteration were in the testicle, before the sarcocele has arrived even to its second stage.

The alteration of the testicle, in such cases, is the original disease, and to be regarded as distinct from sarcocele, and treated according to its particular character. It is not my design to describe the affections proper to the testis; I am only to relate what I have observed of the sarcocele of Egypt. Labourers, but particularly those whose occupation requires their sitting, as weavers, embroiderers, tailors, are most subject to it: many circumstances seem to contribute to produce the disease. Among the internal causes may be enumerated, depraved humours, inveterate siphylis, which has for one of its symptoms in this country, pustules and pruritus of the scrotum, which is, however, much disregarded by the Egyptians. This singular virus, produced also perhaps by vicious humours, is, probably, the cause of another disease not less distressing,—elephantiasis. I have remarked that all those who are affected with sarcocele have

symptoms,

symptoms, more or less apparent, of elephantiasis. The subject of a case at the end of this memoir is a striking example. All these causes produce their effects on the cellular membrane and skin of the scrotum, as being most disposed to the attacks of psoric and similar complaints: its laxity, its innumerable mucous cryptæ, and the little sensibility it is endued with, predispose it to tumefaction; the vessels first are surcharged, become weakened in their tone, the scrotum enlarges, and at the same time acquires a density like that of the placenta. The testicle preserves its form and health, but soon ceases to be distinguished, except at the posterior part of the tumour, which continues to increase in every direction, but particularly downwards. The cellular membrane thickens as well as the envelopes of the testicle; and the skin also augments in density and size. The integument which covers the pubis, the inguina, the penis, and the nates, contributes gradually to form the enormous tumour which is produced; thus the skin covered with hair, which protects the pubis, descends considerably below that region. The extremity of the prepuce presents itself in the form of a sort of navel on the anterior surface of the tumour. The urine flows down from the aperture without being projected. The external surface of this fleshy mass becomes rugous and scaly; it retains but little warmth, and the superficial veins are considerably enlarged. Sarcocele may increase to almost any size: a case related in the German Ephemeris weighed more than two hundred weight. The case of a fellah (agricultor) of Upper Egypt, which I shall hereafter relate, was considered to weigh one hundred pounds. I have seen in different parts of Egypt ten or twelve different cases. If these tumours are dissected, they seem composed of a dense (*couenneuse*) substance, with little vascularity, and harder in some parts than in others; these have but little sensibility, and when cut do not give much pain. This I was able to remark on extirpating a commencing sarcocele in a cook of the Capuchin convent at Cairo. At the School of Medicine at Paris, there is a model of a sarcocele, which was not extirpated, but the dissection after death confirms this description. The testicles were sound, and the tumour was formed by their membranes, then preternaturally distended.

An old man, at Cairo, consulted me for an enormous sarcocele which he had had for twenty years. the size of which obliged him to keep his bed. Anxiety to be relieved had impelled him to consult the physicians of his country, who had exhausted their efforts in vain. Cautery, caustics,

 incisions,

incisions, and discutients, had been employed. The last person he consulted pierced the tumour through its centre with a large needle and ligature; he suffered but little pain, which proved that the testicles did not participate in this disease. This seton, which was moved daily, produced an abundant flow of serous fœtid matter; (he was also affected with elephantiasis). The continued discharge of the seton produced but little diminution of the tumour; and as no more was to be expected from it than other means which had been employed, I proposed the extirpation; and when I was about to perform it, an order to go to Alexandria, which the English had threatened to attack, compelled me to leave this unfortunate old man to his fate. To the causes I have adduced may be added, bad diet, intemperance, too frequent venery, the immoderate use of warm baths, to which all classes of Egyptians are addicted: living in damp and marshy places, the effects of climate, mode of dress, or injuries of the scrotum, may also contribute to the formation of the disease.

Sarcocele has been hitherto considered to belong exclusively to man; the term is limited to the disease in the genital organs. But we may consider the fleshy tumours which take place in other parts, particularly in the face, where the skin is liable, as well as the scrotum, to be affected by venereal and other diseases, as so many sarcomatous tumours of the same nature and depending on the same causes. The examples of such tumours are sufficiently numerous. There are also local causes which determine their formation in one part rather than others, such as falls, mechanical injuries of the skin, or the application of chemical corrosives.

No author, that I know of, has described a similar disease in the female genitals, although the skin which envelops these parts differs but little from the skin of the genital organs of man. The periodical discharge and other resources given by nature to the female, operate, without doubt, against the formation of these excrescences. But a singular case of a woman named Hammet Fatomi, of Cairo, furnishes me with an example of well marked sarcocele of the labia; I shall relate this case. Every author who has written on sarcocele has described it as incurable, from the ill success they have had by internal or topical remedies. All those who have proposed extirpation have been fearful, or at least have not practised it. M. Imbert Delonnes has the merit of being the first to perform this operation, in boldly extirpating the sarcocele of Delacroix; I did

I did not know of the success of his operation when I performed a similar one, (the case I have cited,) and proposed to extirpate several other enormous sarcoceles, when the army was removed.

When the complaint is recent, it may be treated with the remedies hereafter described; but in an advanced stage there is no other resource but extirpation, preceded, however, by remedies proper to remove the cause of the disease. Among the internal remedies antimonials combined with mercurial medicines, and convenient doses of sudorifics, continued for some time, or alternated with small doses of the mineral acids, diluted in some mucilaginous fluid, produce the best effects, but particularly the sulphuric acid, lowered and applied in form of lotion to the parts, or a weak solution of muriate of mercury, or oxide of copper, or muriate of ammonia, the effects of which are increased by gentle and uniform pressure on the disease. The success of these means will be evident in the diminution of the bulk of the tumour, the retraction of the skin, and the amendment of the patient's countenance. If this be the case, the remedies should be continued until the disappearance of the disease. Incisions or caustics seem to me to be useless. I rest my opinion on the little success which the Spanish and English surgeons had in one of the cases related. It is even possible that these means, if followed by the astringent remedies I have mentioned, may produce cancerous ulceration. But after the use of these remedies differently combined, and for a sufficient time, if the sarcocele continues in the same state, I do not hesitate to pronounce the necessity of amputation, and the possibility of performing it without danger. Its necessity is marked by the failure of all other means, and the certainty that the disease will continue to increase; and though the inconveniences are not intense, they lead with certainty to the grave.

It now only remains for me to describe how the operation is to be performed. The vessels which supply the tumour arise from the external pudic artery, and some branches of the internal pudic artery. The spermatic arteries are sent wholly to the testes, and are therefore out of the way; and the hæmorrhage which the other vessels produce is not of much importance, and the arteries are easily secured by a ligature when they are divided. The operation is long and tedious, but not highly painful. The removal of the tumour being complete, if the disease even has been complicated with elephantiasis, which I have usually seen, there

is no fear that the sarcocele will be reproduced; but the remedies for elephantiasis should be continued.

There are some general precepts for this operation: the testes should be carefully avoided, and also the corpora cavernosa and spermatic chords. Two oblique incisions beginning from the prepuce, one passing down below the testis on each side of the tumour. The parts between the testicles and corpora cavernosa must be deeply cut with a double-edged knife, carefully avoiding the testes, and the portion below the line formed by these incisions should be removed. If there still remain some sarcomatous substance round the penis or testes, it should be dissected away. The corpora cavernosa and testes are to be covered by the skin which is left, and the edges may usually be approximated and confined by ligature, or by plaister and bandage. The parts discharge, retire and cicatrize without difficulty. If hæmorrhage occurs, the vessels should be secured immediately with ligature, or (if their orifices cannot be discovered) by actual cautery. The success of the operation will be improved by continuing the use of internal remedies.

Case I.

Jacques Moline, a Copt, and cook to the convent of Capuchins at Cairo, consulted me for a considerable tumour in the scrotum, which he had had for many years; it was of a pyramidal form, and weighed about six pounds. The right testicle corresponded with the superior part of the tumour, and was sound; the penis had almost disappeared; the left testis was confounded with the fleshy mass which formed the sarcocele, and could not be felt: I still doubted if it formed part of the tumour, for he had never felt pain. This swelling was formed of a dense (*couenneuse*), and in some parts almost cartilaginous, substance. In the middle of the irregular mass the testicle was discovered diminished in size; the wound was properly dressed. The treatment was not disturbed by any untoward accident, and on my departure for Alexandria I left the patient advancing to his recovery.

Case II.

Mahammet Ibrahim, about sixty years of age, was blind and affected with the elephantiasis, which he had had for many years. His legs were half as large again as his thighs, and his feet were monstrous. The skin towards the superior part of the leg was smooth and marbled, and there were

many

many varicose veins running on it. The other part was round, with thick yellow rugous incrustations, disposed like scales, separated from each other, particularly at the articulations, by deep ulcerated furrows, which discharged an ichorous fœtid fluid. The crusts were more considerable at the ancles and the heel than other parts. Deep cavities were observable between the toes, and on the sole of the foot. When pressure was made on the parts of the limb which were most swelled, there was no pain produced, nor was any impression left by the finger; the skin and cellular membrane offered all the resistance of cartilage.

This man had lost his sight by the endemic ophthalmia; he was of dark complexion, of weak constitution, and languished out a miserable life. The tumour weighed about seventy-five pounds; it was of an oval form, and interspersed at its inferior part with furrows and incrustations, hard and resisting in some parts, and soft, but without fluctuation, in others, of a blackish brown colour. At the middle and fore part an oblong aperture, surrounded with a thick and callous border formed by the pressure, was observed. This aperture led to the urethra, which passed upwards and backwards towards the pubis. The corpora cavernosa were felt anteriorly at the neck of the tumour, and the testes on the sides, or rather towards the back part; these last seemed sound, and the spermatic chords were elongated and enlarged, and the arteries, whose pulsation was readily perceived, seemed to have enlarged their calibre; the skin of the abdomen was stretched to accommodate itself to the tumour, and the hair of the pubis was considerably below that region, so much indeed that the navel was on the pubis.

This enormous mass, which was supported by a suspensory, produced no other inconvenience than to impede by its weight the motions of progression.

Case III.

A husbandman of Upper Egypt had a sarcocele for twelve or fifteen years, which was daily augmenting. At the time I saw him at Cairo his tumour was enormous, and weighed near one hundred pounds; it descended nearly to his feet, separating the legs; it was of an oval form and of a brownish colour, unequal on its surface, and interspersed with incrustations: like the sarcocele of Ibrâhim, the prepuce was in the middle of the anterior part, and the testicles on its sides, or towards the superior portion. After having been treated by the physicians of his country, he consulted

an English physician who was travelling in Egypt; in hopes of a perfect cure, he consented to the application of an actual cautery; but the repeated use of this remedy gave him no relief, and the tumour continued in the same state. Some years after, he consulted a Spanish physician, who also was journeying in Egypt, who plunged a knife deeply into the tumour, persuaded that it was hydrosarcocele, but there issued only a little blood; and the disease, so far from yielding to this operation, became worse and larger. These two operations, the patient said, were performed without his suffering much pain; and no accident or inconvenience was produced. The cicatrices were yet tender when I first saw him at Cairo, and he was disposed to submit to its extirpation, which I advised, but the same impediments as in the former case prevented my performing it.

Case IV.

Hamet Fatomi, thirty years of age, wife of a labourer of Cairo, came into the civic hospital on account of two tumours which she had had for many years. These tumours seemed to have their origin in the external labia, for there was no vestige of these parts to be seen, nor of the nymphæ; they were nearly of the same size, were placed by the side of each other at the entrance of the vagina, and each of them resembled the head of an infant, rugous and unequal on the greatest part of their circumference, smooth on the inner part, and of a violet colour; their prominent sides, or rather their base, was covered with pustulous incrustations, like the sarcocele of Ibrahim, and discharged a similar fœtid ichorous fluid; they were attached by small roots to the ischium and pubis, were hard, insensible, and like scirrhus, measured about thirteen inches each in circumference, four inches in diameter, and seven inches long. The woman, who was of a sickly constitution, had an incipient elephantiasis. Her lips were thick and of a lead colour, her gums pale and ulcerated, sorrowful appearance of the eyes and countenance, and disposed to melancholy: the digestive functions, however, went on well. I attributed this affection to the incipient elephantiasis, and it is remarkable that she had never menstruated regularly.

I proposed to extirpate the tumour, and began to give her remedies which I had already employed with benefit in elephantiasis. In six weeks her limbs and lips were less swelled, and nearly in their natural state; she had become stronger, the tumours were somewhat softer, the discharge which flowed from the ulcerated scales had become less

fœtid:

fœtid:—in short, I considered her in a fit state for the operation.

The necessity of extirpation in this case, and in the case of Ibrahim, was acknowledged in a consultation, and the operation was fixed for the next day, when the order arrived for me to repair with the army to Alexandria, and obliged me to abandon both these cases.

L. *Improvements in the Aquatinta Process, by which Pen, Pencil, and Chalk Drawings can be imitated. By Mr. J.* HASSELL, *of Clement's Inn**.

SIR, PERCEIVING the various methods of imitating drawings and sketches in the graphic art fall short of an accurate imitation of the black-lead pencil, I determined on an attempt, some years since, which, after repeated experiments, I flatter myself I have fully established.

The manner is totally new, and solely my own invention:—by the method I adopt any artist can sketch with a black-lead pencil his subject immediately on the copper, and so simple and easy is its style, that an artist can do it with five minutes study.

By this manner, the trouble in tracing on oil paper, and other re-tracing on the etching ground is avoided, and the doubtful handling of an etching-needle is done away, as the pencilling on the copper is visible in the smallest touch:—It has also another perfection, that by using a broader instrument it will represent black chalk, a specimen of which I procured Mr. Munn, the landscape-painter, to make a trial of. I have herewith sent the said specimen marked C, and Mr. Munn's name is affixed to the same. This subject he actually drew upon copper, under my inspection, in less than twenty minutes, the time he would have taken, perhaps, to do the same on paper; in fact, it can be as rapidly executed on copper as on paper.

It is particularly pleasant for colouring up, to imitate drawings, as, the lines are soft, and blend in with the colour. It is a circumstance always objectionable in the common method of etching, that those so tinted can never be sufficiently drowned, nor destroyed, and always present a wiry hard effect.

It is equally adapted to historical sketching, and might

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxviii.—The Society's silver medal and thirty guineas were voted to Mr. Hassell for this communication.

be

be the means of inducing many of our eminent painters to hand down to posterity their sketches, which, at present, they decline from the irksome trouble attending the repetition of retracing their performances, and the doubtful handling of the etching needle, which can never give a sufficient breadth and scope to their abilities.

I have, sir, forwarded, in an annexed paper, the different specimens, for the inspection of the gentlemen forming the Society of Arts, &c. &c.

In making my specimens I have thought it necessary to show, if by any accident a part might fail, that it could be re-touched a second time, and oftener if wanted; in this particular its simplicity stamps its use.

To elucidate the foregoing proposition, I purposely caused a part of the distance to fail in specimen AA; this is repaired you will perceive in specimen B, and the sharp touches wanted to perfect the sketch are added.

I beg also to state, it is not the style usually termed *soft ground etching:* that process is always uncertain, cannot be repaired, and will only print about two hundred impressions; whereas the specimens herewith sent will print upwards of five hundred with care.

Should the Society for the Encouragement of Arts, &c. deem the subject worthy of their reward, I shall feel proud in communicating its process, and flatter myself the arts and artists will feel a peculiar addition and pleasure in its utility. Permit me, sir, to subscribe myself, with all respect,

Your obedient humble servant,
JOHN HASSELL,
Landscape-Draftsman, 11, Clement's Inn, Strand.

March 26, 1810.

To C. Taylor, M.D. &c. &c. &c.

Process of drawing upon Copper, to imitate Black-lead Pencil or Chalk.

A remarkable good polish must be put on the copper with an oil-rubber and crocus-martis well ground in oil; after which it must be cleaned off with whiting, and then rubbed with another clean rag.

You are then to pour over your plate the solution to cause ground, which is made as follows:

No. 1.—Three ounces of Burgundy pitch.
One ditto of frankincense.

These are to be dissolved in a quart of the best rectified spirits

spirits of wine, of the strength to fire gunpowder when the spirits are lighted.

During the course of twenty-four hours this composition must be repeatedly shook, until the whole appears dissolved; then filter it through blotting-paper, and it will be fit to use.

In pouring on this ground, an inclination must be given to the plate that the superfluous part of the composition may run off at the opposite side; then place a piece of blotting-paper along this extremity, that it may suck up the ground that will drain from the plate, and in the course of a quarter of an hour the spirit will evaporate, and leave a perfect ground that will cover the surface of the copper, hard and dry enough to proceed with.

With an exceeding soft black-lead pencil sketch your design on this ground, and when finished take a pen and draw with the following composition, resembling ink: if you wish your outline to be thin and delicate, cause the pen you draw with to be made with a sharp point; if you intend to represent chalk-drawing, a very soft nib and broad-made pen will be necessary, or a small reed.

No. 2.—Composition, resembling ink, to draw the design on the copper.

Take about one ounce of treacle or sugar-candy, add to this three burnt corks reduced by the fire to almost an impalpable powder, then add a small quantity of lamp-black to colour it; to these put some weak gum-water, (made of gum-arabic,) and grind the whole together on a stone with a muller: keep reducing this ink with gum-water until it flows with ease from the pen or reed.

To make the ink discharge freely from the pen, it must be scraped rather thin towards the end of the nib, on the back part of the quill, and if the liquid is thick reduce it with hot water.

Having made the drawing on the copper with this composition, you will dry it at the fire until it becomes hard; then varnish the plate all over with turpentine-varnish (No. 3,) of the consistency of the liquid varnish sent with this as a sample.

It will now be necessary to let the varnish, that is passed over the plate, dry, which will take three or four hours at least; but this will depend on the state of the weather; for if it should be intensely hot, it ought to be left all night to harden.

Now the varnish is presumed to be sufficiently hard, you may rub off the touches made with the foregoing described ink with spittle, and use your finger to rub them up; should it

it not come off very freely, put your walling-wax round the margin of your plate, and then pour on the touches some warm water, but care must be taken it is not too hot.

The touches now being clean taken off, wash the plate well and clean from all impurities and sediment of the ink, with cold *soft* water, then dry the plate at a distance from the fire, or else in the sun, and when dry, pour on your aquafortis, which should be in cold weather as follows:

To one pint of nitrous acid, or strong aquafortis, add two parts, or twice its quantity of soft water.

In hot weather, to one part of nitrous acid add three parts of water.

In every part of this process avoid hard or pump water.

The last process of biting in with aquafortis must be closely attended to, brushing off all the bubbles that arise from the action of the aquafortis on the copper.

In summer time it will take about twenty minutes to get a sufficient colour: in winter perhaps half an hour, or more. All this must depend on the state of the atmosphere and temperature of your room. If any parts require to be stopt out, do the same with turpentine-varnish and lamp-black, and with a camel-hair brush pass over those parts you consider of sufficient depth; distances and objects receding from the sight of course ought not to be so deep as your fore-grounds; accordingly you will obliterate them with the foregoing varnish, and then let it dry, when you will apply the aquafortis a second time, and repeat this just as often as you wish to procure different degrees of colour.

Every time you take off the aquafortis the plate must be washed twice with soft water, and then set to dry as before.

To ascertain the depth of your work, you should rub a small part with a piece of rag dipped in turpentine, and then apply the finger, or a piece of rag rubbed on the oil-rubber, to the place so cleared, and it will give you some idea of the depth.

The walling-wax is taken off by applying a piece of lighted paper to the back of the plate, all round the opposite parts of the margin where the wax is placed; then let the plate cool, and the whole of the grounds, &c. will easily come off by washing the plate with oil of turpentine, which must be used by passing a rag backwards and forwards, until the whole dissolves: it is then to be cleaned off by rags; and care must be taken that no part of the turpentine is left hanging about the plate.

The plate should only pass once through the press.

Directions respecting Grounds.

No. 1.—The ground in hot weather must have an additional one-third of spirits of wine added to it for coarse grounds, to represent chalk; and one-half added to it for fine grounds, to represent black-lead pencil; and always to be kept in a cold place in summer, and a moderate warm situation in winter.

N. B.—If any parts are not bit strong enough, the same process is to be repeated.

Gum-water must be made in the proportion of half an ounce of gum-arabic to a quarter of a pint of water.

Turpentine-varnish is composed of an ounce of black rosin to an eighth part of a pint of spirits of turpentine: if the weather is excessively warm, it ought to be made with a sixth part of a pint of spirits of turpentine.

Tracing-rag should be made of a piece of Irish linen, not too much worn, the surface of which is to be rubbed with another rag dipped in sweet oil, just sufficient to retain a small portion of vermilion or pounded red chalk. This must be placed with the coloured part towards the ground of the plate, and the drawing or tracing laid upon it, which must be traced very lightly with a blunt point or needle.

LI. *Method of preparing Ox-Gall in a concentrated State for Painters, and for other Uses.* By Mr. RICHARD CATHERY, *of Mead's Row, Westminster Road* *.

It has been long a desideratum to find out a method of preparing ox-gall for the use of painters, so as to avoid the disagreeable smell which it contracts by keeping in a liquid state, and at the same time to preserve its useful properties. I have invented a method of doing it with very little expense, which will be to those who use gall a great saving, as it will prevent it from putrefying or breeding maggots.

One gall prepared in my method will serve an artist a long time, as it will keep a great number of years. It will be a convenient article for use, as a small cup of it may be placed in the same box which contains other colours, where it will be always ready. The qualities of gall are well known to artists in water-colours, particularly to those

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxviii.——The Society voted ten guineas to Mr. Cathery for this communication.

who

who colour prints, as many colours will not, without gall, work free on such paper, on account of the oil that is used in the printing-ink.

The artists who make drawings in water-colours also use gall in the water which they mix their colour with, as it clears away that greasiness which arises from moist hands upon paper, and makes the colour to work clear and bright. My preparation is ready for use in a few minutes; all that is necessary being to dissolve about the size of a pea of it in a table-spoonful of water.

It is also of great use to housekeepers, sailors, and others, to clean woollen clothes from grease, tar, &c. and will be found advantageous for many other purposes.

If it should meet with the approbation of the Society, I have no objection to prepare it for sale.

I am, sir, your obedient servant,

To C. Taylor, M.D. Sec. RICHARD CATHERY,
Botanical Colourer.

Process for preparing Ox Gall in a concentrated State, by Mr. Cathery.

Take a gall fresh from the ox and put it in a bason, let it stand all night to settle, then pour it off from the sediment into a clean earthen mug, and set it in a saucepan of boiling water over the fire, taking care that none of the water gets into the mug. Let it boil till it is quite thick, then take it out and spread it on a plate or dish, and set it before the fire to evaporate; and when as dry as you can get it, put it into small pots, and tie papers over their tops to keep the dust from it, and it will be good for years.

Certificates were received from Mr. Gabriel Bayfield, No. 9, Park-place, Walworth; and Mr. William Edwards, No. 9, Poplar-row; both botanical colourers; stating, that they have used the ox-gall prepared by Mr. Cathery, and find it to answer better than gall in a liquid state; that this preparation is free from disagreeable smell, and is much cheaper, as one ox-gall thus prepared will last one person for two years, and be as fresh as if just taken from the ox.

A certificate was received from Mr. James Stewart, No. 26, St. Martin's-street, Leicester-square, stating, that he lately belonged to his Majesty's ship the Vestal frigate, and that he took out with him, in a voyage to Newfoundland, a large pot of the prepared ox-gall, for the purpose of washing his greasy clothes for two years; that he found it very serviceable, and to keep its virtue as well as the first day.

LII. *A*

LII. *A Chemical Analysis of Sodalite, a new Mineral from Greenland.* By THOMAS THOMSON, *M.D. F.R.S.E. Fellow of the Imperial Chirurgo-medical Academy of Petersburgh**.

THE mineral to which I have given the name of *sodalite* was also put into my hands by Mr. Allan. In the Greenland collection which he purchased, there were several specimens of a rock, obviously primitive. In the composition of these, the substance of which I am about to treat formed a constituent, and, at first appearance, was taken for feldspar, to which it bears a very striking resemblance.

This rock is composed of no less than five different fossils, namely, garnet, hornblende, augite, and two others, which form the paste of the mass. These are evidently different minerals; but in some specimens are so intimately blended, that it required the skill of Count Bournon to make the discrimination, and ascertain their real nature. Even this distinguished mineralogist was at first deceived by the external aspect, and considered the paste as common lamellated feldspar, of a greenish colour. But a peculiarity which presented itself to Mr. Allan, in one of the minerals, induced him to call the attention of Count Bournon more particularly to its construction.

On a closer examination of the mineral, M. de Bournon found that some small fragments, which he had detached, presented rectangular prisms, terminated by planes, measuring, with the sides of the prism, 110° and 70° or nearly so,—a form which belongs to a rare mineral, known by the name of Sahlite, from Sweden. He further observed, intermixed along with this, another material; and after some trouble, succeeded in detaching a mass, presenting a regular rhomboidal dodecahedron. It was to this form that Mr. Allan had previouly requested his attention.

Some time before this investigation, M. de Bournon had examined a mineral from Sweden, of a lamellated structure, and a greenish colour, which he found indicated the same form. From this circumstance, together with some external resemblance, which struck him, he was induced to conclude that our mineral was a variety of that substance.

To that substance the name of Swedish *natrolite* had been given, in consequence of the investigation of Dr. Wollaston, who found that it contained a large proportion of soda.

* From the Transactions of the Royal Society of Edinburgh.

There

There are few minerals, however, that are so totally distinct in their external characters as the natrolite of Klaproth, and the substance we are now treating of. The mineral examined by Klaproth occurs at Roegan *, on the Lake of Constance, in porphyry-slate, coating the sides of veins and cavities in a mamellated form, the texture of which is compact, fibrous, and radiated; the colour pale yellow, in some places passing into white, and marked with brown zones. Hitherto it had never been found in a state sufficiently perfect to afford any indications of form. Lately, however, M. de Bournon was so fortunate as to procure some of it, presenting very delicate needle-form crystals, which, by means of a strong magnifier, he was able to ascertain presented flat rectangular prisms, terminated by planes, which, he thought, might form angles of 60° and 120° with the sides of the prism. With this, neither our mineral nor the Swedish can have any connexion, further than some analogy which may exist in their composition.

Concerning the Swedish mineral, I have not been able to obtain much satisfactory information. There is a specimen of it in Mr. Allan's cabinet, which he received directly from Sweden, sent by a gentleman who had just before been in London, and was well acquainted with the collections of that city, from which it is inferred, that the specimen in question is the same as that examined by Count Bournon and Dr. Wollaston.

Werner has lately admitted into his system a new mineral species, which he distinguishes by the name of *Fettstein*. Of this I have seen two descriptions; one by Haüy, in his *Tableau Comparatif*, published last year; and another by Count Dunin Borkowski, published in the 69th volume of the *Journal de Physique*, and translated in Nicholson's Journal, (vol. xxvi. p. 384). The specimen, called *Swedish Natrolite*, in Mr. Allan's possession, agrees with these descriptions in every particular, excepting that its specific gravity is a little higher. Borkowski states the specific gravity of *fettstein* at 2·563; Haüy at 2·6138; while I found the specific gravity of Mr. Allan's specimen to be 2·779, and, when in small fragments, to be as high as 2·790. This very near agreement in the properties of the Swedish natrolite, with the characters of the fettstein, leads me to suppose it the substance to which Werner has given that name. This opinion is strengthened by a fact mentioned

* It has been observed also, by Professor Jameson, in the flœtz-trap rocks behind Burntisland.

by

by Haüy; that fettstein had been at first considered as a variety of *Wernerite*. For the specimen sent to Mr. Allan, under the name of *Compact Wernerite*, is obviously the very same with the supposed natrolite of Sweden. Now, if this identity be admitted, it will follow, that our mineral constitutes a species apart. It bears, indeed, a considerable resemblance to it; but neither the crystalline form, nor the constituents of fettstein, as stated by Haüy, are similar to those of the mineral to which I have given the name of Sodalite. The constituents of fettstein, as ascertained by Vauquelin, are as follows:

Silica	44·00
Alumina	34·00
Oxide of iron	4·00
Lime	0·12
Potash and soda	16·50
Loss	1·38
	100·00

II. *Description of Sodalite.*

Sodalite, as has been already mentioned, occurs in a primitive rock, mixed with sahlite, augite*, hornblende, and garnet†.

It occurs massive; and crystallized, in rhomboidal dodecahedrons, which, in some cases, are lengthened, forming six-sided prisms, terminated by trihedral pyramids.

Its colour is intermediate between celandine and mountain green, varying in intensity in different specimens. In some cases it seems intimately mixed with particles of sahlite, which doubtless modify the colour.

External lustre glimmering, internal shining, in one direction vitreous, in another resinous.

Fracture foliated, with at least a double cleavage; cross fracture conchoidal.

Fragments indeterminate; usually sharp-edged.

Translucent.

Hardness equal to that of feldspar. Iron scratches it with difficulty.

Brittle.

Easily frangible.

* This situation of the augite deserves attention. Hitherto it has been, with a few exceptions, found only in flœtz trap rocks.

† The particular colour and appearance of this garnet, shows that the rock came from Greenland: for similar garnet has never been observed, except in specimens from Greenland.

Specific gravity, at the temperature of 60°, 2·378. The specimen was not absolutely free from sahlite.

When heated to redness, does not decrepitate, nor fall to powder, but becomes dark-gray, and assumes very nearly the appearance of the Swedish natrolite of Mr. Allan, which I consider as fettstein. If any particles of sahlite be mixed with it, they become very conspicuous, by acquiring a white colour, and the opacity and appearance of chalk. The loss of weight was 2·1 per cent. I was not able to melt it before the blow-pipe.

II. *Chemical Analysis.*

1. A hundred grains of the mineral, reduced to a fine powder, were mixed with 200 grains of pure soda, and exposed for an hour to a strong red heat, in a platinum crucible. The mixture melted, and assumed, when cold, a beautiful grass-green colour. When softened with water, the portion adhering to the sides of the crucible acquired a fine brownish-yellow. Nitric acid being poured upon it, a complete solution was obtained.

2. Suspecting, from the appearance which the fused mass assumed, that it might contain chromium, I neutralized the solution, as nearly as possible, with ammonia, and then poured into it a recently prepared nitrate of mercury. A white precipitate fell, which being dried, and exposed to a heat rather under redness, was all dissipated, except a small portion of gray matter, not weighing quite 0·1 grain. This matter was insoluble in acids, but became white. With potash it fused into a colourless glass. Hence I consider it as silica. This experiment shows that no chromium was present. I was at a loss to account for the precipitate thrown down by the nitrate of mercury. But Mr. Allan having shown me a letter from Ekeberg, in which he mentions that he had detected muriatic acid in sodalite, it was easy to see that the whole precipitate was calomel. The white powder weighed 26 grains, indicating, according to the analysis of Chenevix, about 3 grains of muriatic acid.

3. The solution, thus freed from muriatic acid, being concentrated by evaporation, gelatinised. It was evaporated nearly to dryness; the dry mass digested in hot water acidulated with nitric acid, and poured upon the filter. The powder retained upon the filter was washed, dried, and heated to redness. It weighed 37·2 grains, and was silica.

4. The liquor which had passed through the filter was supersaturated with carbonate of potash, and the copious white

white precipitate which fell, collected by the filter, and boiled while yet moist in potash-ley. The bulk diminished greatly, and the undissolved portion assumed a black colour, owing to some oxide of mercury with which it was contaminated.

5. The potash-ley being passed through the filter, to free it from the undissolved matter, was mixed with a sufficient quantity of sal-ammoniac. A copious white precipitate fell, which being collected, washed, dried, and heated to redness, weighed 27·7 grains. This powder being digested in sulphuric acid, dissolved, except 0·22 grain of silica. Sulphate of potash being added, and the solution set aside, it yielded alum crystals to the very last drop. Hence the 27·48 grains of dissolved powder were alumina.

6. The black residue which the potash-ley had not taken up, was dissolved in diluted sulphuric acid. The solution being evaporated to dryness, and the residue digested in hot water, a white soft powder remained, which, heated to redness, weighed 3·6 grains, and was sulphate of lime, equivalent to about 2 grains of lime.

7. The liquid from which the sulphate of lime was separated, being exactly neutralized by ammonia, succinate of ammonia was dropped in; a brownish-red precipitate fell, which, being heated to redness in a covered crucible, weighed 1 grain, and was black oxide of iron.

8. The residual liquor being now examined by different re-agents, nothing further could be precipitated from it.

9. The liquid (No. 4), from which the alumina, lime, and iron had been separated by carbonate of potash, being boiled for some time, let fall a small quantity of yellow-coloured matter. This matter being digested in diluted sulphuric acid, partly dissolved with effervescence; but a portion remained undissolved, weighing 1 grain. It was insoluble in acids, and with potash melted into a colourless glass. It was therefore silica. The sulphuric acid solution being evaporated to dryness, left a residue, which possessed the properties of sulphate of lime, and which weighed 1·2 grain, equivalent to about 0·7 grain of lime.

10. The constituents obtained by the preceding analysis being obviously defective, it remained to examine whether the mineral, according to the conjecture of Bournon, contained an alkali. For this purpose, 100 grains of it, reduced to a fine powder, and mixed with 500 grains of nitrate of barytes, were exposed for an hour to a red heat, in a porcelain crucible. The fused mass was softened with

 water,

water, and treated with muriatic acid. The whole dissolved, except 25 grains of a white powder, which proved on examination to be silica. The muriatic acid solution was mixed with sulphuric acid, evaporated to dryness; the residue, digested in hot water, and filtered, to separate the sulphate of barytes. The liquid was now mixed with an excess of carbonate of ammonia, boiled for an instant or two, and then filtered, to separate the earth and iron precipitated by the ammonia. The liquid was evaporated to dryness, and the dry mass obtained exposed to a red heat in a silver crucible. The residue was dissolved in water, and exposed in the open air to spontaneous evaporation. The whole gradually shot into regular crystals of sulphate of soda. This salt being exposed to a strong red heat, weighed 50 grains, indicating, according to Berthollet's late analysis, 23·5 grains of pure soda. It deserves to be mentioned, that during this process the silver crucible was acted on, and a small portion of it was afterwards found among the sulphate of soda. This portion was separated before the sulphate of soda was weighed.

The preceding analysis gives us the constituents of sodalite as follows:

Silica	38·52
Alumina	27·48
Lime	2·70
Oxide of iron	1·00
Soda	23·50
Muriatic acid	3·00
Volatile matter	2·10
Loss	1·70
	100·00

Mr. Allan sent a specimen of this mineral to Mr. Ekeberg, who analysed it in the course of last summer. The constituents which he obtained, as he states them in a letter to Mr. Allan, are as follows:

Silica	36·
Alumina	32·
Soda	25·
Muriatic acid	6·75
Oxide of iron	0·25
	100·00

This result does not differ much from mine. The quantity of muriatic acid is much greater than mine. The lime and

and the volatile matter which I obtained, escaped his notice altogether. If we were to add them to the alumina, it would make the two analyses almost the same. No mineral has hitherto been found containing nearly so much *soda* as this. Hence the reason of the name by which I have distinguished it.

LIII. *Mr.* FAREY's *Reply to Mr.* JOHN TAYLOR *on Water-Pressure Engines for Mines.*

To Mr. Tilloch.

SIR, I SHOULD probably have passed over the observation, at page 394, of your last volume, that though various constructions of water-pressure engines had been attempted, none had yet been very successfully made *on a large scale*, had I not heard the comments thereon by a Cornish mine-owner, very conversant with the mines of that county, which tended to show, that no pressure engines in Cornwall were at all comparable in mechanic effect, with that which he recommended Mr. Trevithick to the erection of at Yolgrave, in Derbyshire; and which brought strongly to my recollection the opinions to a similar effect, which I heard from numbers of the best informed miners in Derbyshire, when I was on my survey of that county; and had more than once heard the particulars stated of the quantity of water lifted, &c.: but having then letters of recommendation in my pocket, to the principal proprietor of the mine where this engine was erected, I purposely neglected, and when at the mine also, to note down the particulars, which the agent mentioned from memory, as I was assured of seeing the working, drawings, and every particular respecting the engine, when I called on the gentleman alluded to at some miles distance: unfortunately this was delayed by circumstances until I was about leaving the district, and when I called, the gentleman was from home: yet still, as a month had passed over without any one of your correspondents having noticed the above assertion, I thought it but an act of justice to an engineer, who is looked upon as having performed an essential service to the mining interests of Derbyshire, but whom I never saw or had any communication with whatever, to state what I did, at page 5, of your present volume. As I am unacquainted with any rule by which to know *how much water* a shallow mine or a deep one may produce, without knowing

any

any others of the circumstances, I cannot see the ground of Mr. Taylor's arguments, if such they can be called, at page 130, to show the probability even, that I had mistakenly confounded *large* and *small*, in recalling his attention to the passage first alluded to; or see, how the words "upon a large scale," as applied to an hydraulic engine, can be limited to the height of the lift, to the neglect of time and quantity of water. I certainly could have had no view, as I had no motive, to disparage Mr. Taylor's invention, in correcting what I thought, and still think, an erroneous general assertion, by stating what particulars my notes on Crash-purse Mine then furnished, as to what had there been done; and I beg now to supply the following, obtained, through the medium of a friend in London, from Philip Stevens who put up the Crash-purse engine for Mr. Trevithick.

Length of the 15-inch pipe which conveys down the water of the Bradford river 44 yards; diameter of the working cylinder 25 inches, and length of stroke therein $10\frac{1}{2}$ feet: the above, by means of a beam 40 feet long, works three pumps, two of whose working barrels are 33 inches diameter, and the other $10\frac{1}{2}$ inches diameter, which each separately lift 17 yards, into the sough, six strokes in a minute, of about seven feet each.

I am, sir,
Yours, &c.
JOHN FAREY.

Westminster, April 16, 1811.

LIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 21.—MR. MACARTNEY furnished a short paper on the stomachs of birds, in which he mentioned his discovering a new organ in the intestines of these animals, calculated to assist their digestive powers.

March 28.—A letter from Mr. John Farey, who has for a long time past been engaged in a mineralogical survey of Derbyshire and its environs, addressed to the President, was read, which, referring to a sketch map of the district, explains the situation of certain enormous *faults* that he had discovered, surrounding large tracts of country in Derbyshire, Nottinghamshire, Staffordshire, Cheshire, Lancashire and Yorkshire, and within which the whole mass of strata (containing coal seams over much of the surface) seem, from a comparison with those of the surrounding district,

district, to have been prodigiously lifted, and to be denudated (or abrupted as Dr. W. Richardson has termed similar operations in Ireland, in the Phil. Trans. 1800, see our 33d vol. p. 114,) or have a very great thickness of the upper strata stripped from off them, and gone. That within this tract other faults partly surround a second tract, which is more raised than the first; and within this again, another tract of limestones and toadstones in Derbyshire and Staffordshire is partly surrounded by other faults, and still more raised and proportionally denudated, so as, in the opinion of the writer, to exhibit lower strata in the British series, than perhaps any other parts of these islands exhibit. The combined effect of these successive lifts, and the general denudation, has been, to exhibit at the southern end of the Weaver Hills in Staffordshire, a far greater vertical derangement of the strata or fault, than has hitherto been mentioned by any writer. A smaller raised tract, similar to the inner one of the above, is also described, on which the town of Bakewell is situated; and it is shown, how the rise of the western edge of this tract and the excavation of the valleys for the Wye, the Lathkil, and the Bradford rivers, occasions the 1st toadstone under the 1st limestone rock (which forms the general surface) to be locally laid bare in these valleys. The great number of names of places and of strata mentioned in this letter, prevents our giving a more detailed account of its contents. From the minuteness with which they are described, it will, we trust, hereafter prove interesting to geological readers.

April 4.—A long paper, by Mr. Jordan, on light and the coloured rings of thin plates, was read, in which the author, after repeating his former objection to the supposed theory of Newton, that light has fits of easy transmission, refuted the theories lately proposed in the Philosophical Transactions; asserted his claim to priority in this inquiry; examined and pointed out the inadequacy of what he called the bow-theory; declared his opinion that, contrary to the ideas of Newton, white light is not a compound, but derives its colours from the objects by which it is refracted; and concluded with promising a new and satisfactory theory of the phænomena of coloured rings in thin plates, which he is about to bring forward immediately. He has pursued these researches for a series of years, published the result of some of them in the Philosophical Transactions for 1799, and has since greatly extended and improved them.

The Society then adjourned, in consequence of the Easter festival, till Thursday, April 25th, when part of an ingenious paper was read on the measurement of the head, by Mr. C.

 Bell.

Bell. This able physiologist took a review of the different methods proposed by anatomists for measuring the dimensions of the head and face, particularly those of Camper and Blumenbach, stated their general insufficiency, and proposed his own method of measuring the facial line from the top of the skull to the point of the upper jaw; and also the depth of the head, by balancing the skull on a steel point, and thus taking the relative proportions and measurement; by which he is enabled to ascertain the precise difference between the heads of Negroes and Europeans, the former being much smaller than those of the latter. Mr. B. also made some curious observations on the dimensions of the organs of taste and smell, tending to prove that their magnitude is no proof of their acuteness.

SOCIETY OF ANTIQUARIES.

Tuesday the 23d of April being St. George's Day, the Society of Antiquaries met at their apartments in Somerset Place, in pursuance of their Statutes and Charter of Incorporation, to elect a President, Council, and Officers of the Society for the year ensuing; whereupon

The Most Noble George Marquis of Townshend and Earl of Leicester,
F. A. Barnard, Esq.
William Bray, Esq.
Nicholas Carlisle, Esq.
The Lord Bishop of Cloyne,
Sir H. C. Englefield, Bart.
Anthony Hamilton, D.D.
Viscount Harberton,
Craven Ord, Esq.
Matthew Raper, Esq.
Rev. Stephen Weston,

eleven of the Council, were re-chosen of the New Council; and

Edward Astle, Esq.
John Caley, Esq.
Rev. Jas. Dallaway,
Lord Dundas,
Hon. R. F. Greville,
Joseph Jekyll, Esq.
Charles Monro, Esq.
George Saunders, Esq.
John Symmons, Esq.
Rev. H. J. Todd,

ten of the other Members of the Society, were chosen of the New Council, and they were severally declared to be the Council for the year ensuing; and on a Report made of the Officers of the Society, it appeared that

The Most Noble George Marquis of Townshend and Earl of Leicester was elected President.

William Bray, Esq. Treasurer,
Matthew Raper, Esq. Director,
Rev. T. W. Wright, A.M. Secretary, and
N. Carlisle, Esq. Secretary for the ensuing year.

The Society afterwards dined together at the Crown and Anchor Tavern in the Strand, according to annual custom.

ROYAL

ROYAL SOCIETY OF EDINBURGH.

March 4.—Mr. Allan read a paper on the rocks of the environs of Edinburgh, being the first of a series which he proposes to read on that subject. The present one embraced the rocks of St. Leonard's Hill and Salisbury Craig. The specimens illustrating the subject he presented to the Society to be deposited in their Cabinet.

March 18.—Sir George Mackenzie read some geological remarks on the appearances presented by different rocks in Iceland; and showed their importance in connecting the phænomena of volcanoes with the principles of the Huttonian theory. Sir George brought forward the results of Sir James Hall's experiments on heat modified by compression, and successfully applied them to support his conclusions. The facts were explained in a satisfactory manner; and the whole paper was so important in a geological point of view, that we regret it is not in our power to give an analysis of it. We understand, however, that it will form a part of the account of Iceland, which Sir George and his friends are about to publish. The work is now in the press.

April 1.—Dr. Brewster read a description of a new instrument for measuring capillary attraction. The instrument is to be exhibited at a future meeting.

Professor Playfair read a most interesting paper, being part of his new edition of his illustrations of the Huttonian theory, entitled Remarks on the Natural History of Volcanoes.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society, on the 16th of February, Professor Jameson read a mineralogical description of the romantic and interesting valley of Pentland, near Edinburgh. After describing the magnitude and direction of the valley, and the shape and height of the mountains surrounding it, the Professor stated as a probable conjecture, that it was formed by a lake which had emptied itself by a lateral valley which traverses its south side. He next enumerated and described the various rocks of which it is composed, and gave a full detail of their geognostic relations. Four sets of rocks were described as occurring in this valley: transition rocks,—sandstone and conglomerate rocks,—rocks of trap-tuff,—wacke,—amygdaloid, &c.; and rocks of the claystone series, as claystone, porphyroid, compact-feldspar, &c.

At the meeting on the 9th of March, the Secretary read commu-

communications from Dr. Thomas Thomson, containing an analysis of iron-ore from Greenland,—from Dr. Edmonston, Shetland, on the larus glaucus,—and from Dr. Barclay on the structure of the cells of bees and wasps.

At the meeting of the Society on the 6th of April, Mr. William Elford Leech read an account of the natural tribe of diptera, the eproboscidea of Latreille, with descriptions of the species, which he illustrated by drawings and specimens. —— At the same meeting, Professor Jameson gave an account of the occurrence of coal in the first sandstone formation in Thuringia and other countries on the continent; from whence he inferred the probability of that valuable mineral existing in the extensive red-sandstone districts of Scotland.

Since our last, the first volume of the Memoirs of the Wernerian Natural History Society has been published.

NEW BERLIN UNIVERSITY.

The new Berlin university was opened for the first time on the 1st of November 1810. It is divided into four *faculties*, as is the case with most of the German universities. The palace of Prince Henry of Prussia, which was given by the king for the use of the university, has been fitted up in a commodious manner, and divided into ten large lecture-rooms, with a hall for the profesors to assemble in. The rest of the palace has been converted into galleries and museums. M. Rudolphe has been appointed to the superintendance of the cabinet of comparative anatomy and zoology. The superb mineralogical cabinet of the late M. Karsten forms one of the most valuable acquisitions to the institution; and has been put under the care of Professor Weiss, late of Leipsic, who is also lecturer on mineralogy. Count Hoffmanseg, author of the superb work entitled *Flora Lusitanica*, has presented to the institution some very fine subjects in natural history, particularly some rare articles from the Brazils, America, and the South Seas. Dr. Gerresheim of Dresden has given a very fine cabinet of zoophytes, which has been committed to the charge of M. Ilger of Brunswick. The king of Prussia has purchased M. Herbst's collection, and intends to purchase several others. M. Willdenow has reorganized the garden of plants at Berlin, and after visiting Paris for the purpose of adding to his botanical collection, will resume the chair at Berlin as lecturer on that science.

Hermstadt lectures on chemistry, Tralles and Iabba Oltmans on mathematics and astronomy. The observatory of the

the university will be under the direction of M. Tralles. The grand royal observatory, which Messrs. Bode and Ideler superintend, will also be connected with the university, as well as the academy of arts and sciences. The faculty of medicine will be under the direction of Messrs. Reil (of Halle) Hufeland, and Bernstein. M. Savigny, from the university of Landshut, will teach the civil law; and some professors of jurisprudence have been invited from Gottingen, Heidelberg, Jena, and Leipsic. M. Sewalz, already known for several excellent political works, will lecture on politics. M. Sartorius of Gottingen, and M. Wilken of Heidelberg, will lecture on history. Messrs. Fichte and Schleyermacher will be the professors of rational philosophy, and M. Wette of Heidelberg will teach theology. All the other branches of science will be taught by esteemed masters, as soon as they can be procured.

The university will have a rector, a senatus academicus, and all the institutions usual in the German universities. The professors and students will be subject to academical jurisdiction. They will not be amenable to civil jurisdiction, until they have been solemnly deprived of their academical privileges.

LV. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir, Having read in the Philosophical Magazine Dr. Lettsom's account of the happy effects of *oleum terebinthinæ rectificatum* in expelling the tænia, I was induced to make trial of it on an unfortunate woman, whose affliction, arising from the tape-worm, during five or six years, had reduced her almost to death, notwithstanding the applications which had been made by several medical men.

Though the quantity of oil administered to her did not exceed five drachms by measure, she evacuated within half an hour a tape-worm $5\frac{1}{2}$ yards in length. In consequence of this, I was soon applied to by another woman in a similar state: the same quantity was prescribed as in the former case, without any immediate effect; and about nine hours afterward the dose was repeated, which produced a laxative motion with a discharge of tænia.

Considering it a duty I owe to Dr. Lettsom to make this communication; should you deem it worthy a place in your estimable Magazine, you will confer an honour on,

Sir,—Your obliged and obedient servant,

Gainsborough.

L. Towne.

Dr.

Dr. Adams is preparing a Syllabus of his Course to assist such of his hearers as are less acquainted with Mr. Hunter's doctrines, or unaccustomed to apply them to medicine.

An academy for the encouragement of science and literature has been established in the Ionian Republic. A prize of 600 francs in value has been lately offered by M. Lesseps, the imperial commissary for the Ionian islands, for the best essay concerning some important branch connected with the statistics of these islands. Another prize of 600 francs has been offered by M. Teotochi, president of the Ionian senate, for the best elucidation of the following question :

"What are the easiest methods for rendering the crops of corn and potatoes abundant in the Isle of Corfu?"

Candidates may write their papers in Italian, French, Greek or Latin, and must transmit them to the Secretary of the Ionian Academy, on or before the 1st of July 1811, in the usual manner, with sealed mottoes, &c.

LECTURES.

Theatre of Anatomy.

Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations ; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Summer Course will commence on Saturday, May 25, 1811, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had, on applying to Mr. Taunton, Greville-street, Hatton Garden.

Dr. Adams's Summer Course of Lectures on the Institutes and Practice of Medicine will commence in the end of May or beginning of June.

LIST OF PATENTS FOR NEW INVENTIONS.

To Henry James, of Birmingham, in the county of Warwick, for an improvement in the method or mode of navigating, forcing, towing and hauling boats, barges, and other vessels, upon canals, rivers, and other navigable waters, by means of mechanism or machinery to be worked by steam or any other suitable power.—March 26, 1811.

To Thomas Deakin, of St. John-street, in the parish of St. Sepulchre without, in the county of Middlesex, stove grate-maker, for an improvement in kitchen ranges and stoves, and in the mode of setting the same.—April 1.

To Thomas William Sturgeon, of Howland-street, in the county of Middlesex, esq. for certain improvements on a micrometer.—April 1.

To Samuel Bentham, of Hampstead, in the county of Middlesex, civil architect and engineer of the navy, for his secure and œconomical mode of laying foundations, and in some cases of proceeding with the superstructure of works of stone or of brick or other artificially composed materials, particularly applicable to the projection of wharfs and piers into deep water, to the construction of bridges, and to the formation or improvement of harbours, as well as to the erection of heavy buildings on bad ground.—April 2.

To Cornelius Varley, of Junction Place, Paddington, in the county of Middlesex, artist, for his new telescope or optical instrument for viewing distant objects, and for other useful purposes; with a suitable table or stand for the same. —April 5.

To John Blenkinsop, of Middleton, in the county of York, coal-viewer, for certain mechanical means by which the conveyance of coals, minerals and other articles is facilitated, and the expense attending the same is rendered less than heretofore.—April 10.

To John Taylor, of Greenwich, in the county of Kent, esq. for improvements in the construction of wheels for carriages of different descriptions.—April 11.

To William Finch, of Birmingham, in the county of Warwick, iron-master, for his method of making nails of wrought-iron.—April 11.

To John Brown, of New Radford, near Nottingham, lace net-manufacturer, for his machine or machines for the manufacture of bobbin-lace or twist-net similar to and resembling the Buckinghamshire lace-net and French lace-net as made by the hand with bobbins on pillows.—April 24.

To

To John Stockwell, of the city and county of Bristol, for certain improvements in the art or method of manufacturing shag tobacco, whereby the stalks taken out of the leaf tobacco may be cut up into shag tobacco without injuring the quality thereof.—April 24.

To William Bundy, of Camden Town, in the county of Middlesex, mathematical instrument-maker, for his improvement on stringed musical instruments.—April 24.

To John Bradley, of Colborn Hill, in the hamlet of Amblecoat, in that part of the parish of Okeswinford which lies in the county of Stafford, iron-master, for his new method of manufacturing gun-skelps.—April 24.

Rain Table, by the Rev. J. BLANCHARD, *of Nottingham.*

1810.	Bristol.	Chichester.	London.	Chatsworth, Derbyshire.	Derby.	Horncastle, Lincolnshire.	Ferriby, Kingston-upon-Hull.	Heath, near Wakefield, Yorkshire.	Manchester.	Lancaster.	Dalton, Lancashire.	Kendal.	Feltfoot, near Milnthorpe, Westmoreland.	Carlisle.	Nottingham.
Jan.	no ac.	0·28	0·26	0·58	1·10	1·14	0·64	0·89	1·39	2·17	2·85	2·68	4.87	1·84	1·05
Feb.	0·90	2·90	1·44	1·15	1·84	1·64	1·10	1·95	2·57	1·91	2·54	4·15	3·11	1·22	1·03
Mar.	2·30	2·84	2·54	2·10	1·50	1·71	0·94	3·45	3·19	2·37	6·03	4·26	8·00	3·80	1·40
April	1·68	1·61	1·70	1·92	1·33	0.82	1·54	1·91	1·92	0·37	1·12	1·03	2·30	1·04	1·00
May	1·42	1·46	1·04	2·89	3·20	2·40	2·66	3·13	1·41	0·12	0·75	0·81	0·60	0·53	2·60
June	2·59	0·49	0·56	0·87	1·42	1·54	1·27	1·90	1·90	1·47	1·87	2·10	1·92	1·60	1·18
July	1·55	4·72	3·78	2·23	3·01	3·50	3·77	4·41	5·50	3·14	3·89	3·49	4.55	3·24	3·85
Aug.	4·52	3·07	2·46	2·92	3·40	4·13	4·33	3·18	5·00	3·58	4·18	4·54	4·75	3·22	2·61
Sept.	2·66	1.95	1·98	2·13	1·85	0·10	0·58	2·10	1·90	2·58	2·62	2·07	2·60	1·70	0·62
Oct.	2·66	3·31	1·92	1·73	2·52	2·40	2·21	1·88	4·68	4·00	4·70	3·97	5·43	3.12	2·72
Nov.	3·45	11·77	6·08	4·59	6·16	5·23	5·95	5·12	3·68	4·50	5·10	4·01	4·86	2·15	3·02
Dec.	6·80	4·53	2.94	4·87	2·26	3 47	3·95	4·30	6·03	6·47	7·19	8·41	8·28	4·30	2·07
Total	30·53	38·93	26·76	27·98	29·59	28·08	28·97	34·22	39·17	32·68	42·84	41·52	51·27	28·76	23·15

Meteorological Table, by Dr. Clarke, *of Nottingham.*

Thermometer.				Barometer.				Weather.		Winds.			
Maximum.	Minimum.	Medium.	Greatest Variation in Twenty-four Hours.	Maximum.	Minimum.	Medium.	Greatest Variation in Twenty-four Hours.	Fair.	Wet.	N. and N. E.	E. and S. E.	S. and S. W.	W. and N. W.
53	18	36	10	30·36	29·75	30·05	0·29	26	5	5	9	17	8
54	14	37	16	30·34	28·73	29·75	0·68	19	9	7	4	21	7
59	30	43	10	30·10	28·88	29·62	0·41	19	12	17	9	14	11
70	32	47	9	30·18	29·27	29·76	0·33	24	6	14	7	18	2
68	29	47	15	30·33	29·05	29·86	1·05	23	8	26	4	7	6
78	38	57	10	30·35	29·72	30·38	0·35	28	2	16	7	15	4
77	42	57	15	29·95	29·40	29·75	0·31	12	19	9	4	12	5
80	40	57	10	30·43	29·39	29·79	0·52	21	10	1	6	18	6
82	39	56	11	30·38	29·71	30·10	0·31	28	2	17	6	10	5
68	24	45	8	30·30	29·03	29·86	0·54	23	8	15	11	8	6
53	26	38	10	30·12	28·86	29·44	0·55	25	5	11	10	8	7
50	19	36	10	30·50	28·85	29·62	0·71	21	10	5	2	9	21

ANNUAL RESULTS AT NOTTINGHAM.

Thermometer.	Wind.
Highest Observation, September 2d	82°E.
Lowest Observation, February 20th	14°NE.
Greatest Variation in Twenty-four Hours, February 19-20	16°
Annual Mean	46°
Barometer.	**Wind.**
Highest Observation, December 31st	30·50NE.
Lowest Observation, February 19th	28·73SW.
Greatest Variation in Twenty-four Hours, May 20th	1·05
Annual Mean	29·83

Weather.	Days.
Fair	269
Wet	96
	365

Winds.	Times.
N. and NE.	143
E. and SE.	79
S. and SW.	157
W. and NW.	88
	467

Rain.	Inches.
Greatest Quantity in July	3·85
Smallest ditto in September	0·62
Total Quantity for the Year	23·15

The Barometer is firmly fixed to a standard wall on an elevation of 130 feet, and the Pluviameter is placed in a garden 140 feet from the level of the sea.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For April 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	40	56°	37°	30·35	55	Fair
28	34	58	41	·44	72	Fair
29	42	52	38	·52	63	Fair
30	38	59	41	·24	65	Fair
31	37	49	37	·15	31	Cloudy
April 1	45	52	38	29·95	30	Fair
2	44	60	50	·81	30	Fair
3	45	62	44	·92	58	Fair
4	48	60	41	30·01	42	Fair
5	43	52	34	·01	32	Fair
6	34	56	48	29·67	41	Fair
7	36	41	35	·45	10	Sleet
8	32	40	32	·42	15	Sleet
9	32	46	33	·61	26	Fair
10	31	51	32	·79	65	Fair
11	32	49	39	30·03	63	Fair
12	40	50	45	·24	62	Fair
13	46	63	51	29·99	32	Showery
14	54	64	52	30·16	36	Cloudy
15	55	62	55	·16	40	Cloudy
16	54	61	47	29·82	0	Rain
17	45	60	50	·65	35	Fair
18	50	55	51	·25	26	Stormy
19	51	59	50	·16	46	Stormy
20	51	60	52	·32	41	Stormy
21	52	60	55	·60	55	Fair
22	55	66	54	·55	46	Fair
23	56	71	60	·60	66	Fair
24	56	72	55	·75	49	Fair
25	55	61	54	·75	51	Fair
26	54	64	52	66	62	Fair

N.B. The Barometer's height is taken at one o'clock.

ERRATUM.—In Mr. Donovan's Paper, p. 246 of the present Number, for Plate V *read* Plate VI.

LVI. *A* Table *of the Beats, on Mr.* William Hawkes's *Patent Organs and Piano-Fortes, calculated by the Rev.* C. J. Smyth; *and communicated, with some Remarks, by Mr.* John Farey.

To Mr. Tilloch.

Sir, Considerable discussions occurred some time ago, in Dr. Kemp's quarto Musical Magazine, published monthly, on the merits and defects of the patent Instruments with 17 sounds in each octave (or *dixseptave*) and a single pedal, invented by Mr. W. Hawkes, and on their comparative merits, with other patent Instruments more recently invented by Mr. David Loeschman, having 24 sounds in the octave (or *vingtquatreave*) and six pedals: without it appearing from such discussions, what the precise temperament is, which Mr. Hawkes has now adopted. That gentleman also published a small pamphlet in the last year, on his "Improved Musical Scale;" from which it was easy enough to discover, that he had abandoned his former temperament, of $\frac{1}{5}$th of a major comma as the flat temperament of 9 of his fifths, (the beats of which system were calculated by Mr. Barraud from my Theorems, as given at page 129 of your present volume), and had, at page 14, adopted "$\frac{1}{6}$th of a comma," as the degree of temperament, which he has "made choice of for his patent organ and piano-forte:" but, from *three* different commas, viz. the major comma $= 11\,\Sigma + \text{m}$, the artificial comma of Mercator (in that particular case where it measures the $\frac{1}{53}$d part of the octave, for in all other situations it has *different meanings*) $= 11{\cdot}58106\,\Sigma + \text{m}$, and the comma of Pythagoras (or Diaschisma) $= 12\,\Sigma + \text{m}$, having been mentioned by Mr. H. in the preceding pages, it was impossible to discover from his pamphlet, which of these commas he meant; which induced me to write to Mr. Hawkes, at Newport in Shropshire, to point out the different commas above referred to, and to request to know, which of these commas was to be divided into six parts for his temperament; and his answer, dated the 23d of February last, says, "I most undoubtedly mean Mercator's comma of $\frac{1}{53}$d of a diapason, which, from its approximation to the truth, and the inexpressibility of any difference to the auricular organ, it may be supposed the true comma."

This information I communicated to my able and zealous friend in these pursuits, the Rev. Mr. Smyth of Norwich, who has calculated and sent me the inclosed Table of Beats

 of

of this system, which you will oblige him and me, by inserting in your Magazine.

It may be necessary to point out, that only the 17 sounds (or 18 by repeating the octave) which are placed in the first range of column 2, are found on Mr. Hawkes's instruments, although they are furnished with 24 strings or pipes, as many as Mr. Loeschman has, but who, by his more numerous pedals, renders them all effective.

Mr. Loeschman has adopted the Mean-Tone System, with the major Thirds *perfect*, the beats of which system, as far as 12 notes, are given by Mr. Smyth in your last volume, p. 436: but in applying which to Mr. L.'s vingt-quatreave, all the large numbers of beats, for the wolves, must be omitted, and other numbers of beats calculated and used in their stead. At a future time I hope, that my friend Mr. Smyth will give a supplement to his mean-tone Table, for supplying this defect to those possessed of and wishing to tune Mr. L.'s instruments: as also to those, who by means of the beats, may be desirous of contrasting the merits and defects of these two patent inventions *for improved Tempered Scales*.—The Rev. Mr. Liston's patent, it will be observed, embraces a very different object, that of *rendering all temperament unnecessary*, and his present instrument, by means of 20 pipes (producing 60 sounds in each octave, by help of 10 pedals) certainly goes much nearer to his object than Mr. Hawkes has approached his; but Mr. Liston's pipes must be increased to 24 and his pedals to 12 (as he proposes to do on future Instruments, if desired) to be enabled to produce perfect harmony to the same extent of modulation, as Mr. Loeschman's instruments, as now made, are capable of rendering, truly tempered scales, according to the mean tones, to Dr. Smith's Equal Harmony, or any other regular system of temperament whatever; and so that no wolf or substituted note shall occur, through 33 keys: a point gained, which certainly would have astonished and delighted the late Dr. Smith (to whom we owe all accurate temperament), compared with the Instruments he was able to produce or procure for his Tempered Scales.

It will be observed, that only the 12 ordinary finger-keys in use, are used in any of these three patent instruments: to each of the inventors of these I profess myself a well-wisher, as far as they obtain their respective objects, and are disposed to pursue the same, with fairness and liberality towards the others, and am, sir, your obedient servant,

Westminster, April 13, 1811. JOHN FAREY.

MR. HAWKES'S

Mr. Hawkes's Dixseptave.

Vibrations and Beats in one Second.

Finger Keys.	*Notes.*		*Vibrations.*	3.	III.	4.	V.	6.	VI.
13	c		480	16·8950	8·9040	4·1892	3·1360	14·1980	14·1612
13		B*		52·7600				62·1260	
12		c b			37·0530		14·1694		42·0502
12	B		450·6862	15·8632	8·3160	3·9382	2·9438	13·3296	13·2984
11	B b		448·5307	15·0822	8·0377	3·7402	2·7998	12·6756	12·6425
11	A*		423·1629	14·8954	34·7887	3·6924	13·3041	12·5162	39·4815
10		B bb			33·0801				37·5405
10	A		402·3608	14·1618	7·4641	3·5112	2·6289	11·9004	11·8707
9	Ab		382·5802	42·0502	7·0990	3·3404	2·4986	49·5116	11·2882
9	G*		377·7874	13·2934	31·0630	3·2996	2·4662	11·1692	35·2522
8	G		359·2160	12·6425	6·6648	3·1360	2·3464	10·6230	10·5980
8		F**		39·4815				46·4880	
7	Gb		341·5570	37·5405	6·3378	14·1694	2·2302	44·2020	10·0780
7	F*		377·2787	11·8707	6·2581	2·9438	2·2033	9 9756	9·9505
6	F		320·6982	11·2882	5·9502	2·7998	2·0946	9·4836	9.4614
6		E*		35·2522		13·3041		41·4596	
5		Fb			24·7558				28·0962
5	E		301·1130	10·5980	5·5846	2·6289	1·9666	8·9040	8·8842
4	Eb		286·3105	10·0780	5·3115	2·4986	1·8701	37·0530	8·4475
4	D*		282·7240	9·9505	28·2140	2·4662	1·8462	8·3610	26·3800
3	D		268·8254	9·4614	4·9878	2·3464	1·7556	8·0377	7·9316
3		C**						34·7887	
2	Db		255·6102	28·0962	4·7418	2·2302	1·6702	33·0801	7·5411
2	C*		252 4082	8·8842	20·7298	2·2033	1·6498	7·4641	7·4477
1	C		240	8·4475	4·4520	2·0946	1·5680	7·0990	7·0809

N. B.—In the vacant spaces the vibrations and beats are the same as those in the line immediately above or below, answering to the same finger-key, designated by the number in the first column.

Norwich, April 8, 1811. C. J. Smyth.

LVII. *Description of a New Thrashing Mill. By* H. P. Lee, *Esq. Maidenhead Thicket* *.

Sir, I beg leave to state to the Society of Arts, &c. the following particulars, relative to my attempts to improve the thrashing machine for corn, and of my success therein.

Being largely concerned in agriculture, and having 800 acres of arable land, I found that a thrashing machine or

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxviii.—The gold medal of the Society was given to Mr. Lee for this invention.

two became absolutely necessary for the continuance of my occupations. I accordingly erected one of the kind recommended to me; but from the complication of its structure, its being frequently out of order, and from its bad performance of the work at all times, I resolved to try to have a thrashing machine made under my own directions, more simple in its construction, and more efficacious in its operations. With this view I have continued my experiments for nearly three years, at an expense of about three hundred pounds, and have, at last, brought my machine to a degree of perfection which is satisfactory. Many gentlemen and farmers who have seen it and its operations, give it a decided preference to any they have seen, for the simplicity of its construction, for the cleanness of its thrashing, and for the quantity of corn thrashed by it, in proportion to the power applied.

I have no doubt but that the result of my original thoughts and experiments on this subject, will be of great advantage in this highly useful agricultural implement, and I have sent a model of the machine for the Society's inspection.

I am, sir,

Your very obedient servant,

Maidenhead Thicket, Dec. 27, 1809. H. P. Lee.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. Lee's *Thrashing Machine, Pl.* VII, *Fig.* 1. *and* 2.

Fig. 1 and 2 are a side and end-view of the machine; A, in both figures, represents the framing of the machine; B is the shaft of a cog-wheel C, which is turned by cog-wheels, from the great horse-wheel, in the same manner as the ordinary thrashing mill; the cog-wheel C turns a small pinion D, to which it gives a rapid revolution; on the axis of the pinion the beaters EE are fixed, and revolve with it, within a segment or drum, formed of iron plates, grooved or ribbed, parallel to the axis, as the figure represents, and connected together by wooden curbs FF, to which they are screwed. *aa* is the feeding board upon which the corn is placed to enter the machine. The end of this board is fixed very near to the four vanes, or beaters, *bbbb*; as these revolve rapidly they strike the heads of the corn upwards, with such a jerk as to beat out all the corn from those ears which they meet fairly; but if any escape they are drawn in, together with the straw, and rubbed round by the beaters against

against the inside of the ribbed drum, or cylinder F, so as to open the ears and let out the corn, though the ears come in any position whatever. At H is a grating, upon which the beaters deliver the corn, chaff, and straw altogether; the two former fall through upon the ground at X, and the latter slides down on the grate; the corn is afterwards to be dressed in a winnowing machine, which separates the light and heavy corn from the chaff. The curbs F are fixed by screws, which can be adjusted so as to bring the cylinder nearer, or further from, the beaters, to adapt the machine for thrashing different kinds of grain; for it is evident that large corn, as peas, beans, &c. must require more space to rub them in than the smaller grain, as wheat and barley. L, fig. 1, is one of the uprights of the frame which supports the bearing for the axis B of the cog-wheel; and M is an oblique brace, which strengthens the frame. N is the stage on which the man who feeds the machine stands.

⁎ This communication was accompanied with various certificates from most respectable individuals largely concerned in the agricultural line; all agreeing that Mr. Lee's thrashing mill is superior to any they had before seen.

LVIII. *On the recent Improvements made in musical keyed Instruments, with Copies of the Specifications of three Patents lately granted for these Purposes, to Mr.* Hawkes, *Mr.* Loeschman, *and Mr.* Liston.

THE interest which has been and seems further likely to be excited, by the recent improvements in musical Intruments, and the probability that now appears, of the mathematical theory of musical Intervals receiving a more direct and useful application to general practice, than has hitherto been the case, I am induced to lay before my readers copies of the specifications enrolled in the patent offices, by Mr. William Hawkes, Mr. David Loeschman, and the Rev. Henry Liston, for explaining their three several inventions; which being here brought together, the nature and peculiarities of each may the easier be understood by my readers, and their several merits appreciated. EDITOR.

Specification of the Patent granted to Mr. William Hawkes, *of Newport, in the County of Salop, for Improvements on musical keyed Instruments with Twelve fixed Tones.*— Dated July 25, 1808.

To all to whom these presents shall come, &c.—Now

 know

know ye, that in compliance with the said proviso, in the said letters patent contained, I the said William Hawkes do hereby declare, that my said improvements on musical keyed instruments of twelve fixed tones, are described as follows:—that is to say, The improvements in the organ are effected by a pedal under the key-board, and an extra slide to every stop in the sound-board, to correspond with the general slide. The above extra slide has a communication from the sound-board to the extra pipes; namely, sharps and flats; which by depressing the pedal with the foot, brings on the sharp scale, and by elevating the pedal brings on the flat scale; and as the flats go off the sharps are brought on; and inversely, as the sharps go off the flats are brought on; by which action of the pedal, communicated to the additional slide with double holes adapted to the additional pipes, namely, five pipes to each octave. My improvement in the piano-forte is effected, by adding seven diatonic and five flat tones to our present scale of twelve fixed tones, which form two chromatic scales; the one termed a flat scale, and the other a sharp scale, and is done by two sets of strings, of two unisons to each set, which are acted upon without the addition of a key to the key-board, by a pedal, by which the key-board is made to move forwards or backwards about one-fourth of an inch, the same hammer striking each set of strings both in the flat and sharp scale, by depressing the pedal with the foot, when the sharp scale is wanted, and elevating the pedal when the flat scale is wanted.—In witness whereof, &c.

Specification of the Patent granted to Mr. David Loeschman, *of Newman-street, in the Parish of St. Mary-le-bone, in the County of Middlesex, Piano-Forte Maker, for Improvements in the musical Scales of keyed Instruments with fixed Tones, such as Pianos, Organs, &c.*—Dated July 26, 1809.

To all to whom these presents shall come, &c.—Now know ye, that in compliance with the said proviso, I the said David Loeschman do hereby declare, that my said invention is described in and by the drawings (see Plate IX) and description thereof hereunto annexed, and in manner following; that is to say,—The scale of a piano-forte or organ on the common principle having 12 sounds within the octave, I have by my invention extended to 24 distinct sounds, which enables the performer to play in 33 perfect keys, 18 major (thirds) and 15 minor thirds; and this I have effected by means of six pedals, that cause the hammers

mers to act upon 24 distinct sets of strings or unisons. Three pedals bring on the flats to be treble, and the like number bring on the sharps to the bass. By reversing my mechanism, I produce also the same effect; in which case, by three of the pedals the flats are brought on to the bass, and by the other three the sharps are brought on to the treble. Every pedal has a separate movement and spring, which act independent of the key; on each movement are fastened two of the twelve hammers belonging to each octave throughout the compass; so that a pedal for the flats brings on two additional flats in each octave, and in like manner a pedal for the sharps brings on in each octave two additional sharps; when such additional flats or sharps are no longer wanted, by omitting the use of the pedal the spring belonging to it immediately leaves the movement to its former position or fixed tones, of three sharps, two flats, and seven natural notes in each octave: the mechanism for the flats and sharps is so constructed, that if more sharps or flats are wanted than one pedal will produce, a second without the first, will be sufficient to bring on two of each in addition. So also, if more sharps or flats than the second pedal will produce are wanted, the third without the first or second, is sufficient to bring on two of each in like manner. Each pedal is made to fasten, if it should be wanted. In Organs, the improvement is effected also by six pedals; and in each octave there are 24 distinct sounds, from 24 distinct pipes: there is a separate movement and a spring to every pedal. Every fixed key has two stickers, two black-falls, and two pallets, which act on two pipes of different sounds. Three of the six movements are fixed in the middle of the front, above the keys, and bring on the sharps to the back of the organ, and the same number are fixed in the like direction behind, to bring on the flats towards the front. By fixing all the six movements in the middle of the front above the keys, or in the same situation behind, I produce also the effect desired. By reversing the movements, my mechanism will admit of bringing on the sharps and flats, either to the front or back of the organ, or, if wanted, my mechanism will admit having both the sharps and flats, either before or behind the instrument. With regard to the pedals and their construction, action, &c. I refer to that part (before described) relative to such, in my piano-forte specification.——In witness thereof, &c.

Specification of the Patent granted to Henry Liston, *of Ecclesmacham, in the County of Linlithgow, Clerk, and Charles Broughton, of the City of Edinburgh, Writer to the Signet, for Improvements in the Construction of Organs.*——Dated July 3, 1810.

To all men to whom these presents shall come, &c.—Know ye, that in compliance with the said proviso, we the said Henry Liston and Charles Broughton do hereby declare, that our invention consists: *first,* in causing each organ-pipe to afford several tones differing from each other in acuteness or gravity, by applying to the mouth of a pipe or to the open end of an open pipe, one or more moveable shades, which are performed by means of a pedal or pedals, or by a stop or stops for the hand, or in any other way, may be enabled to remove from, or bring to the mouth or open end of the pipe at his pleasure. These shades are made of thin plates of lead or pipe-metal, such as is used in the manufacture of metal pipes (thicker or thinner according to the size of the shade) or of other convenient materials. The shades bear a different proportion to the mouths or open ends of the pipes to which they belong, according to the degree of alteration intended to be produced on the pitch of the pipes. When it is intended to alter the pitch of a pipe, by what is called the enharmonic fourth of a tone or the diesis in a tempered system, then the shade is of such size, as to cover the whole length of the mouth (across the pipe) rising about as much above the upper-lip, or of such size as to cover the whole open end of the pipe, and one such shade only is applied to each pipe; or the pitch of an open pipe may be altered, the diesis, by means of one shade at the mouth to alter it in part, and another shade at the open end, to alter the pitch as much more as requisite. This is chiefly useful, when, as sometimes happens, the pipe cannot well bear to be altered the whole diesis at the mouth or open end, or in the case of open wooden pipes, which are tuned by means of a fixed shade at the open end. When it is intended that each shade should alter the pitch by what is called comma, being the difference between the major and minor tones in a system of perfect intonation, then there may be two shades to the mouth or open end of each pipe, and the one shade is made to cover a little more than the half of the mouth across the pipe, but rising as much as the formerly described shade above the upper-lip, or a little more than the half

half of the open end of the pipe; and the second shade is made to cover the remainder of the mouth or open end: or in the case of an open pipe, one shade may be applied to the mouth to alter its pitch comma, and another shade may be applied to the open end to alter it another comma. For the convenience of being removed from or brought to the pipe, the shades are fixed on rollers or cylinders of wood, or other proper materials moving on pivots. For the purpose of attaching the shades to the rollers, each shade may be soldered to a piece of tinned wire, or brass-wire; which piece of wire may be screwed into the roller, or the shade may be attached to the roller in any other convenient way. When two shades are applied to the mouth or open end of one pipe, or when in a range of pipes the shades of some are upon different rollers from those of others, then the rollers may be arranged one above another; the wire or stalk by which the shade is attached on the lower roller, bending round the other roller or rollers, so as to apply the shade close enough to the mouth or open end of the pipe to which it belongs; or when convenient, one or more rollers may be placed on one side, and the others on the other side of a range of pipes.

This description is illustrated in the following figures: (see Plate IX.) No. I. figure 1, shows two pipes shaded at the mouth, each with the single shade, to alter the pitch by diesis: *a, a, a, a,* the mouths of the pipes; *b, b, b, b,* the shades represented transparent, that the mouths may be seen; *c, c,* the wire or sticks by which the shades are attached to the rollers *d d*; the stalk attached to the lower roller, bending round the roller above it; *e, e,* supports for the pivots of the rollers, one at each end of the range of pipes.

Fig. 2 and 3 are side-views of a pipe shaded at the mouth, to show the positions of the shades when close applied, and when removed; the reference being the same as in fig. 1.

Fig. 4, shows two pipes shaded at the open end; *a, a, a, a,* the open ends; *b, b,* the shades; *c, c,* the stalks which attach the shades to the rollers *d, d*; which are represented sloping, to correspond with the tops of the pipes. The shade attached to the upper roller is represented as close applied to the open end of the pipe, and that on the lower roller as removed from the pipe, to which it belongs.

Fig. 5, is a bird's-eye view of the same.

Fig. 6, is a pipe having two shades at the mouth, each to alter the pitch comma.

Fig.

Fig. 7, is a pipe with two shades at the top or open end, the one being close applied, and the other removed.

Fig. 8, is a bird's-eye view of the same. The references in these figures will be understood, from what has been already said. The use of these moveable shades is, that by means of them organs can be constructed with more complete scales than those in ordinary use, without so great a multiplication of pipes, as without this invention would be necessary. And whereas, from the ordinary construction of the bellows, those at least which rise on four ribs of equal breadth at each side, and each end, they do not blow with an uniform force, but with less force when full, and with a continually increasing force as the top sinks, and *vice versâ*.

We, the said Henry Liston and Charles Broughton, declare, that our invention consists, *secondly*, in a regulator which renders the blast of the bellows perfectly equable. This regulator is shown, No. II. fig. 1. it consists of a spiral piece of wood (or other proper material) *a*, 1, 2, 3, of about half an inch in thickness, more or less according to the size of the machine, and of a pulley *b*, 1, 2, 3, of similar thickness fastened to the spiral. This machine turns on a pin at the common centre of the spiral and pulley C. The string *d*, *d*, *d*, is fastened to the pulley *b*, 1, 2, 3, at *c*, and being wound round it, passes under a small pulley at *f*, and is fastened to the top-board of the bellows at *g*, at about an equal distance from either end. Another string *h*, *h*, fastened to the string *d*, *d*, *d*, passes under a pulley at *i*, goes under the bellows, and a similar pulley on the other side, and is fastened to the opposite side of the top-board; that when these strings act on it, they may pull both sides equally. These three small pulleys may run in one piece of wood, as shown, fig. 2, which being placed under the bellows, may be fastened to the frame of the organ; the weight or counterpoise *l* is suspended by a string *m*, at the centre of the spiral, when the bellows are quite empty. When, therefore, the bellows begin to rise, the strings *d*, *d*, *d*, *h*, *h*, are drawn so as to turn the pulley, and consequently the spiral in the direction *b*, 1, 2, 3, and then the string *m* is taken upon the edge of the spiral which is grooved to receive it: thus as the bellows are gradually losing force, the counterpoise *l* is gradually gaining power, by the increasing radius of the spiral on which it acts. If the bellows rise so much as to cause the pulley and spiral to make an entire revolution, the weight *l* will be in the position

position λ, acting on the extremity of the spiral; and as the bellows sink, the spiral turning in the opposite direction, will gradually unwind the string so that the counterpoise will act on a radius continually decreasing, as the force of the bellows is increasing: the accuracy with which the regulator will equalize the force of the blast, depends on three circumstances; First, the form of the spiral, the size of the pulley *b*, 1, 2, 3, and the weight of the counterpoise *l*: the spiral curve is to be formed by the following rule: Describe a circle of any convenient diameter, and supposing the whole circumference to represent the size of the greatest angle which the ribs of the bellows make with the bottom- or top-boards. Assume any point, and from thence divide the circumference into segments (always measured from the same point) respectively proportional to the sines of the angles up to that greatest angle. It will be sufficiently accurate to take the sines of the five first degrees, thence the sines of each half degree up to 15°, and thence to the greatest angle (the entire revolution) each quarter degree, draw radii to all these points. Then from the centre of the circle, measure off each radius proportionally to the secant of its respective angle: and from this point draw a perpendicular to the radius; these perpendiculars by their mutual intersections, will form an irregular polygon, approaching to the curve required. The scale of equal parts by which the radii are measured proportionally off, to the secant of the angles, will be greater or smaller, according to the size of the bellows to be regulated. For a chamber organ of four or five stops, the secant of the greatest angle may be about ten inches: for large bellows it may be considerably larger; otherwise the weight or counterpoise might be inconveniently great: the size of the pulley is to be such that its circumference shall be exactly equal to the rise of the bellows, when the ribs make with the bottom- or top-boards, the greatest angle for which the spiral is made: the size of the pulley will therefore, *cæteris paribus*, depend on the breadth of the ribs—Thus, if the breadth of the rib be five inches, the circumference of the pulley should be equal to twice the size of the greatest angle, as put down in the ordinary table of sines, tangents, &c. calling the first figure in the table, inches, and the rest, decimals of an inch. If the ribs be less or more than five inches, the circumference of the pulley will be found from the tabular sine by a statement in the Rule of Three. First, as five is to the tabular of sine, so is the rib to a fourth proportional; which being doubled, is the circumference required,

quired. The weight of the counterpoise will be most easily found by experiment. If it be too little, it will correct the evil in part but not entirely; the bellows will therefore still gain some force as they fall. If again the counterpoise be too great, the bellows will have most force when full, and will gradually lose force as they fall.—In testimony whereof, &c.

LIX. *Method of producing Heat, Light, and various useful Articles, from Pit-Coal. By Mr.* B. Cook, *of Birmingham**.

Sir, Having paid much attention to the procuring of gas and other products from pit-coal, I now beg leave to lay before the Society for the Encouragement of Arts, &c. the results of some of my experiments on pit-coal, and the methods of procuring the sundry articles of which I have sent samples, and a japanned waiter varnished therewith. The quantity of clear tar which may be produced from every hundred weight of coal is about four pounds, from which a liquor, or volatile oil, may be distilled, which answers the purposes of oil of turpentine in japanning. Every gallon of tar will produce nearly two quarts of this oil by distillation, and a residuum will be left nearly, if not quite equal, to the best asphaltum. I have sent a waiter, or hand-board, japanned with varnish made from this residuum, and the volatile oil above mentioned. This dries sooner, and will be found to answer as well as the best oil of turpentine, a circumstance which will be of immense advantage to this country, as in the vicinity of Birmingham only, nearly ten thousand tons of pit-coal are coked or charred per week; and all the tar hitherto been lost; but by my process, I dare venture to say, that from the various coal-works in this kingdom, more tar might be produced than would supply all our dock-yards, boat-builders, and other trades, with tar and pitch, besides furnishing a substitute for all the oil of turpentine and asphaltum used in the kingdom, and improving the coke so as to make iron with less charcoal.

I have sent a large specimen of the asphaltum, and three vial bottles containing as follows:

No. 1.—A sample of the oil or spirit, being part of that which

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1810.—The Society voted their silver medal to Mr. B. Cook for this invention.

which was used in making the varnish with which the waiter sent was japanned.

No. 2.—Is the same oil or spirit, a little more rectified.

No. 3.—The same, still further rectified, and of course more clear, and freer from smell; but I find that the specimen, No. 1, answers quite as well for varnish.

Tar-spirit is now about 8*s.* per gallon, and turpentine-spirit about 15*s.* This latter has been, within the last two years, as high as 48*s.* per gallon, and the tar-spirit will answer equally well for varnish, on using the coal-tar-spirit, instead of the turpentine-spirit.

I requested Mr. Le Resche to use the tar-spirit just in the same way he would the foreign spirit, and then give the varnish to his work-people to use, without making any remark to them, which was done: he making the varnish himself, found it mixed, and made the varnish as good in appearance as that prepared with the foreign spirit. He then gave the varnish to his work-people to use; and when they had finished their work with it, he found from their report, that it answered perfectly, and dried sooner; and when the waiter done with it was given to the polisher, it was found to polish much smoother under the hand, and take a more beautiful gloss than their former varnish, as the article now sent will show on inspection.

I am of opinion that the production of these articles will be of great public service. Permit me to add, that the timber of ships paid with this tar is not nearly so liable to be worm-eaten as those done with common tar.

I remain, sir,

Your humble servant,

Birmingham, Jan. 12, 1810. B. Cook.

To C. Taylor, M.D. Sec.

Reference to Mr. Cook's *Apparatus for preparing Gas and other Products from Pit-Coal, Pl.* 3.

A, Fig. 1, Pl. VIII, is a common fire-place, a stove built with brick, having cast-iron bars to put the fire in at, and a flue that goes into a chimney; A is the cast-iron pot, (which holds from twenty-five pound to one hundred pound of coal, according to the size of the premises to be lighted) which hangs by the bewels or ears on a hook, suspended by a chain in this stove or furnace, about three inches above the bars of the grate, and three inches distant from the sides of the stove; the fire then flames all round this pot, and as it does not rest on the burning fuel, it is the flame only

that

that heats it, so that it does not scale, but will last for years. The smoke, &c. is carried off into a chimney. The cover *d* of the pot is made rather conical, to fit into the top of the pot close, and from the top of the cover the elbow-pipe proceeds as far as the mark *a*. The other end of the pipe with the elbow entering the water-joint is riveted to it after; when the lid or cover of the pot is put on, the bewels or ears come over the elbow of the pipe that is on the lid, and a wedge is put between them and this elbow, to keep down the cover air-tight, and a little clay or loam may be luted in the joint, if any gas should escape round the cover of the pot. The other elbow B goes into a water-joint, formed of a tube affixed to the cover of the purifier C; and another tube, which passes through the lid of the purifier: the elbow-pipe then goes over the inner tube, and when put on, the jointing is made good by pouring water into the space between the tubes, which renders it air-tight. The gas, as the arrows show, passes down into the purifier C, which is rather more than half full of water; the use of this water-joint is for the convenience of removing the lid *d*, to which this pipe is attached. The purifier C is a wooden trough, with a sheet-iron top, to which the tubes are soldered, and it is fastened to the trough to keep all secure and air-tight. The sheets of iron, *e*, *f*, *g*, *h*, *i*, *k*, are alternately soldered to the iron top, and fastened to the wooden bottom. Now when the trough is half filled with water, the gas passes into it at B; and as it can only find its way out again at R, it must pass through the water. The inner pipe B reaches under the surface of the water in the trough; now when the gas is forced into the water, it would rise to the top of the purifier, and go along in a body to the end, and out at the pipe R, if the sheets of iron, *e*, *f*, *g*, *h*, *i* and *k*, which stand across the trough with openings in them, alternately at top and bottom, did not stop it, force it to descend down into the water, and hinder it from going any way but through these apertures, purifying it all the time it is passing through the whole body of water, until it is properly washed: it then escapes through the pipe R at the end of the trough C, then passes down the pipe S, and is carried up into the reservoir or gazometer K. In the bottom of the purifier is an aperture, closed by a plug at D, to let off the ammoniacal water and tar as it is deposited, and the pipe, with the cock E at the top of the purifier, is to burn away the spare gas when not to be used.

There is a stop-cock placed in the main pipe at F, that

when the reservoir is full, and gas is making, and cannot be used, the cock may be turned, and prevent any gas from passing from the reservoir; and by opening the cock E on the top of the purifier, and firing it, all the gas which is made more than is wanted for use may be burnt away. If this was not done, the gas would continue to find its way into the reservoir K, which would overflow, and produce a disagreeable smell, which this simple way of burning it away as fast as it is made when not wanted, prevents.

It may in some measure happen, that although the gas has passed through the purifier C, yet that a small portion of tar will pass along with it, and would either clog the pipe S, or accumulate in the reservoir. To avoid this, there is placed at the bottom of the pipe S at G, before it rises into the reservoir, a jar into which a pipe made, as shown in the drawing, conducts the tar; this collects all that passes through the purifier; it is filled with water, over which the gas passes up into the reservoir, but the tar drains down this lead pipe and deposits itself in the jar of water. The longer this pipe S is, the better, as it serves as a refrigeratory. H is a plain cask, made to any proper size, and filled with water, with a cock to draw off the water when it becomes foul. The upper vessel K is made of sheet iron, riveted together in the manner engine-boilers are made. If it is only from five hundred to one thousand gallons in size, it will require only two cross iron bars at top, and four ribs down the sides to keep it in form, with a strong ring at top; and as there is no stress on this vessel, it will ascend and descend easily without any other support or framing, the plain sheet iron sides being riveted to the four ribs, and it is quite open at the bottom. A strong rope runs over the pulleys L L, with a weight M to balance the vessel K, and assist it in rising and falling. The pipe J is that through which the gas passes from the reservoir or gazometer, and rising through the pipe T, is conveyed to all parts to be lighted. There is also another drain pipe at N, for after all the washing, &c. a very small portion of tar and moisture may rise into the pipes, and perhaps in time clog them; but by laying all the pipes in the first, second, and third stories on a small descent, if any tar or moisture should rise, it will drain down all the pipes from top to bottom, and be deposited in the earthen jar at N: by that means the pipes will not clog up in half a century. These jars must be sometimes removed and emptied, fresh water put in, as also the water in the vessel H must be changed, to keep it clean and sweet; and the water in the purifier C should

should be changed every two or three days: by these means the gas will be deprived of all its smell, at least as far as washing will effect it, and the apparatus will be clean.

The stop-cock at O is for the use of a master, if he wishes to lock up the gas in the reservoir, to prevent his workman, &c. wasting it in his absence; as also if any pipe should leak, or a cock be out of order, in any part of the premises, by turning this cock all the gas is kept in the reservoir while the pipe is repaired, or any other alteration made; it also extinguishes all the lights when turned, if any are left burning by careless workmen, nor can they be lighted until it is opened again.

The whole of this apparatus is simple, and not liable to be put out of order in such a way, but that any person may put it to rights again. All the art required to make the gas is to take off the cover of the pot, and without removing the pot to take out the coke, and fill it with fresh coal, wedge it down by putting an iron wedge between the bewels or ears and the elbow of the vessel, and if required, plaster a little clay or loam round the cover, to keep it air-tight; a fire is then made under it, and the whole is done. The boy or man who does it, must now and then look at the fire and keep it up, until the pot is hot, and the gas is made. Now in works where lights are wanted almost always, I would recommend two fire-places, and two pots, so that when one pot is burned out, the other pot may be ready to act; for this purpose the purifier must be provided with two of the water-joints B, one communicating with each pot, and the elbow pipe of each pot must have a stop-cock, as V: now when one pot is burning, the cock in the other pipe must be stopped, that the gas may not find its way out of the purifier; and when all the gas is extracted from that pot, the cock C, leading from it, must be stopped, and the pot left to cool; while a fire is put under the other pot, its cock is opened, and a supply of gas from it is passed into the reservoir: by these means one of the pots is constantly supplying the reservoir with gas, and the lights are always kept burning; one purifier is all that is necessary; the cock V must be shut when either of the covers is taken up to fill the pot again with coal: when the elbow-pipe is lifted out of the water-joint, as the cover is attached to it, a plug must be provided to fit into the water-joint pipe the moment the elbow is removed from it, or the gas will rush out of the pipe at the water-joint; but a better way would be, to lengthen the pipe of the water-joints B, and place a large cock

cock under each of them, almost close to the top of the purifier; when one pot is burnt out by turning the cock it keeps all the gas in the purifier while the cover is removed: no plug is necessary in this method. When people are very particular, (especially when houses or accompting-houses are to be lighted,) and wish all smell to be destroyed, if they are not satisfied with washing it, and still think there is a little smell left, (and very little indeed, if any, will be left,) after the washing, a small trough may be added, made in the same way as the purifier, with sheets of iron across to force the gas through the pipe R communicating with it. This trough may be filled with water, with a few lumps of lime put into it, and this water and lime changed often. On the gas being forced through this lime-water, if there was any remaining smell in it, this would completely take it away; and, as has been before observed, by changing all the waters now and then, and keeping this small trough constantly supplied with clean water and lime, the gas after passing it will ascend the pipes to the lights pure.

Sir,—Having been from home, I was prevented from answering your obliging letter until this day.—I am much pleased that the Society have approved of my specimens produced from pit-coal. I feel also highly gratified and honoured with their reward. I hope to lay before you, in a short time, an account of the establishment of a work that will be of such magnitude, as will supply this part of the country with the oil or spirit, in sufficient quantity to supersede the use of turpentine, &c. in japanning; and I do hope that in time works of the same description will be established through all Staffordshire, whose products will supply the place of a great portion of the spirit used in the kingdom, while the pitch will be of sufficient quantity to form a great part of that article now used in the dock-yards.

All I want is support from the great coal companies and masters, to erect sufficient apparatus at the different works to preserve the tar at all the coke furnaces, and proper means to separate the spirit from the tar. It would be a great saving to the nation; as in every one hundred and twelve pounds of coal coked, there is lost by the present mode about four pounds of tar, and the cokes are not half so good as if they were coked in close vessels, to the exclusion of the atmospheric air. I need not describe the method by drawings of the manner of extracting the tar from pit-coal in

close vessels, as that method is so generally known; it must be clear to every one, that it is procured by distilling the coal.

I have, as follows, described the method I use in extracting the spirit from the tar, the process of which is so simple that every one must understand it.

Fig. 2, Pl. VIII. is a section of the furnaces, and one of the retorts, almost any number of which may work in a line; the same flue will do for all, only taking care, if any are not at work, to stop up the draught-hole, which communicates with the flue. These furnaces are built without bars, grates, or doors. A is the place where the fuel is put in to heat the retort G; the fire lies under it, and the smoke is carried off into the flue F. B is the aperture where the ashes are raked out. G is a section of the iron bason, or lower part of the retort; the dark-shaded square part shows the space the fire occupies, and the black square D the flue as it runs along the back of all the line of furnaces, and enters the chimney R, as the arrows show. I, figs. 2 and 3, shows the upper part of the iron, earthen, or glass retort, fitted on the cast-iron bason G. K, the receiver. By this mode of setting the retorts, all the great expense of bars, doors, frames, &c. is saved, and a brisker draught of air is obtained, which may be slackened at pleasure by covering up in part, or wholly, the fire-place A with a brick. E is a square iron plate with a circular hole in the centre, built on the top of the furnace. The cast-iron bason of the retort G is made to the size of the hole in the plate: the most convenient size of the bason of the retort I find is about five or six gallons, in the shape of a deep pot, with a flanch or rim H round the edge of it; this pot or bason of the retort is put into the iron plate E, and the flanch of the retort then rests on the plate E. I is the upper part of the retort without a bottom, made to rest and fit on the flanch of the cast-iron bason G. K is the receiver, larger in the mouth than the nose of the retort. To begin the work, I fill, nearly, the iron bason of the retort G with coal-tar. I then put on the upper part of the retort I, and make it air-tight with a little sand thrown round it at the flanch H; the receiver K is put in its place, and a slow fire is put in at A, under the retort: the tar soon begins to boil slowly, or rather simmer. Now as soon as that begins there rises from the tar a thick whitish vapour, which fills the glass retort, part becomes condensed, and falls in drops from the sides of the retort into the tar again, while the purer spirit rises into the neck, is condensed, and keeps dripping down the

the neck into the receiver: this is the spirit of the tar, and with this spirit that first arises from the tar was the waiter japanned which I sent you. The reason I chose to have the receiver wider at the mouth considerably than the nose of the retort is, that there is a strong and very volatile oily ammonia, that does not soon condense, but gets out of the receiver into the air the instant it leaves the retort, and though but in a very small quantity, so small that it is hardly possible to catch it; yet will it impregnate the air for a great distance round, with its very penetrating smell, while the spirit keeps dropping into the receiver pure and separate from the ammonia. The spirit is very volatile, quite as much so, if not more, than the spirit of turpentine, and soon evaporates if exposed to the air, which is a proof of its drying nature; indeed when used as a substitute for turpentine, it dries in the stove quite as soon or sooner, and takes equally as beautiful a polish. I sent you three specimens. No. 1. is what came off the tar first. No. 2. is the same distilled a second time; and the third specimen is the second re-distilled again in a glass retort: it there leaves a little pitchy residuum, and comes over clear, as the sample. Very little of the spirit is lost in passing through these different stages, if care is taken that the fire is slow and the process not hurried. When the spirit is perfectly extracted from the tar, there remains in the bason of the retort that beautiful pitch or asphaltum sent, which when mixed with the spirit forms an ingredient for making the black varnish used in japanning. If it is wished to use it as pitch, less spirit must not be extracted from it. I find that six gallons of tar will produce, if care is taken, about two gallons or two gallons and a half of spirit. A great number of retorts may be kept working by a single man; if we say only one hundred, and only worked down in a day, they will produce two hundred to two hundred and fifty gallons of spirit, so that, by increasing the number, any quantity may be obtained. When the spirit is used in the place of turpentine, the varnish-maker uses it in the same way, and in the same quantity, as there appears no manner of difference in the use of it from spirit of turpentine in the making of varnish. When the asphaltum is used, it supplies the place of real asphaltum, and in about the same quantity. I have explained the whole as clear as I can; but if any more information is required, I should feel happy in giving it, and am, sir, with great respect,

Your obedient humble servant,

Birmingham, March 13, 1810.

B. Cook.

To C. Taylor, M.D. Sec.

LX. *Comparative Analysis of the Gum Resins. By* H. Braconnot, *Professor of Natural History, &c. &c.**

ANALYSIS OF CAMBOGE.

§ I. It is generally understood that this gum is procured by incision from the bark of the camboge, a large tree in India, the top of which is extremely woody, and the trunk ten or twelve feet in circumference. This tree, which Linnæus designates by the name of *Cambogia gutta*, requires the greatest heat of our hot-houses when we wish to produce it in Europe by means of its seeds, which are contained in a pulpy acid fruit, of the size of an orange.

Camboge is brought in different-sized rolls from the kingdom of Siam, China, and the island of Ceylon; and was scarcely known in Europe until the commencement of the 16th century. The authors who have mentioned it do not agree respecting its most remarkable physical properties. Thus, while they allow it to be of an orange-yellow colour without smell, some assign to it a slight acrid taste, and others on the contrary assert that this taste is very strong: for my own part, (and I have examined a great number of specimens of the best quality,) I found that all of them had an almost insipid taste.

A like difference of opinion has prevailed respecting the analytical results which have been given of the substance in question. According to Cartheuser, it contains more extractive than resinous parts: he gives this, however, only as a conjecture, and thinks it very difficult to separate these two principles. Geoffroy is of a contrary opinion: for he says, in his *Materia Medica*, that camboge contains five-sixths of resinous substances, and one-sixth only of extractive parts soluble in water. But we shall presently see that it contains no extractive at all.

§ II. If we expose camboge to the flame of a candle, it swells and takes fire like resinous matter: on heating it in a cup it exhales a particular smell, softens, and will be decomposed rather than enter into fusion.

Fifty grammes when distilled produced—1st, A brown water in which was empyreumatic acetous acid.—2d, A small quantity of light oil.—3d, There afterwards passed over in considerable quantity another heavy thick oil of a brown colour. There remained in the retort a light coal

* *Annales de Chimie*, tome lxviii. p. 33.

of

of the weight of eight grammes, the incineration of which left five decigrammes of ashes, which produced,—two centigrammes of potash partly sulphated, four centigrammes of phosphate of lime, six centigrammes of carbonate of lime, and three decigrammes of quartzy sand, containing a little charcoal and some traces of oxide of iron. Lime and the alkalis did not discover any ammonia in the liquid products of this distillation.

§ III. Twenty grammes of camboge were treated with warm alcohol and filtered. There remained on the filter a substance which when well washed with alcohol was a grayish colour, drying with difficulty, and becoming brittle. In this state it weighed exactly four grammes: it had an almost insipid taste, and was entirely dissolved in water, with the exception of one decigramme of impurities. This solution reddened turnsole: when evaporated to dryness, it left a transparent friable residue similar to the coloured gum of the plum-tree, which like the latter burns with little flame, and leaves a good deal of compact charcoal, in which we find phosphate of lime.

The alcoholic solution was of a red colour; when evaporated to dryness it yielded a resin weighing 16 grammes. This resin is transparent, of a red colour, without any marked taste, and having a distinct idio-electrical virtue; when pulverized it gives out a particular smell, and assumes a bright yellow colour.

If into the saturated solution of this resin in alcohol we pour some water, a little heat is produced, and a yellowish milky liquor is formed; whereas most of the other resins precipitated from alcohol by water terminate by becoming partly grumous: it is on account of this easy division of the resin of camboge that it is employed so much in miniature-paintings, &c.

§ IV. The solution of potash acts upon the resin of camboge with great promptitude, particularly when it is warm: there results an oily-like liquor of a deep red, in which the properties of potash are neutralized: on evaporating this combination almost to dryness, it crystallizes like the solutions of aloes.

The soap of camboge resin is of a deep red colour, and almost black: it is soft to the touch, becomes friable when dried, and resembles a resin.

It has the taste of rancid grease, and leaves a slight degree of acrimony on the tongue. It is easily dissolved in water without disturbing it.

The acids make so abundant a deposit in the soapy

solution

solution of camboge, that the whole liquor goes into a thick coagulum of a fine yellow colour.

Lime-water produces in this soap a precipitate of a fine orange colour.

The earthy salts also form yellow precipitates in it as well as most of the solutions of white metals. It precipitates the sulphate of iron in brown, and the nitrate of copper in green.

§ V. Ten grammes of camboge were put into a retort with 80 parts of the strong nitric acid used in commerce. As soon as the retort received the impression of the fire, red vapours were produced, the intensity of which soon disappeared. I poured into the retort the first products which had passed during the operation, which was continued until the matter was dissolved, and thickened to the consistency of a syrup*: on cooling, there was formed a mass of lamellated crystals, enveloped in a viscous matter: the whole was diluted with a quantity of water, which produced a sediment which when well washed and dried weighed one gramme three decigrammes.

This substance was of a yellowish colour, of a bitter taste, and was partly dissolved in boiling water: the solution became turbid on cooling, and deposited part of the dissolved matter on filtering the liquor. It is of a reddish yellow colour, becomes frothy by agitation, reddens turnsole tincture, assumes a deeper colour on its mixture with the alkalis, and slightly precipitates at the end of a certain time the sulphate of iron.

When put upon the fire, this matter does not melt so easily as the resin of camboge: it gives out an aromatic smoke, and leaves plenty of charcoal.

It unites very well with potash and spirit of wine, and red transparent solutions result.

The nitric acid when slightly heated with this substance dissolves it without altering it sensibly: water produces in the solution an abundant white coagulum.

From these properties I think myself warranted in regarding this substance as a particular species of soluble factitious resino-bitter, united with a yellowish resiniform matter insoluble in water.

* The produce of this distillation, when well saturated with chalk and distilled again, furnished a liquor slightly acid, of an extremely pungent smell and of a very strong bitter. The alkalis colour it slightly in yellow. On adding sulphate of iron to this mixture, a precipitate is formed entirely soluble in the acids. There is no prussic acid therefore in this liquor, the nature of which is not well known to me.

The

The acid liquor and the washings were thickened in order to drive off what remained of the nitric acid, and this residue was diluted with water in which had been dissolved a small quantity of potash, which separated four decigrammes of the yellow resiniform substance. The liquor when again thickened, and then treated with alcohol and filtered, left some very white acidulous oxalate of potash, which when dried weighed one gramme. The alcoholic solution furnished on evaporation three grammes of bitter matter soluble in water, and which contained malic acid.

§ VI. I diluted some camboge in fine powder in a certain quantity of water, and passed through it a stream of oxygenated muriatic gas, with the intention of taking away the colour of this resin, which in fact lost its fine yellow colour: the milky liquor, when thickened, afterwards diluted in water and filtered, left on the filter a substance which was washed with boiling water until the latter did not redden turnsole tincture. The following were the properties which this substance presented:—

It is pulverulent, of a pale yellow colour without any sensible taste: it crackles under the teeth like an insoluble salt, nor does boiling water dissolve it. It is very little fusible, and does not give out any smell while it is not decomposed; but when we set fire to it, or when we spread it on burning coals, it exhales pungent vapours of muriatic acid.

The weakened acids give out nothing perceptible from it; but if they are concentrated, there is a production of charcoal and muriatic acid.

When joined with potash this substance yields a compound of an agreeable smell of soap, in the solution of which the nitrate of silver forms a precipitate partly soluble in the nitric acid.

I distilled six grammes of this substance in a small retort which I made red-hot: the produce was collected in some decigrammes of water, which when examined at the termination of the distillation was strongly acid, and had the smell of muriatic acid: I poured into this water some nitrate of silver, which produced an abundant precipitate of muriate of silver. When well dried and washed, this precipitate weighed five grammes four decigrammes, which contain 1 gramme 35 centigrammes of muriatic acid, supposing, according to Bergman, that 100 parts of muriate of silver are formed of 25 muriatic acid and 75 oxide.

There remained in the retort two grammes one decigramme of light charcoal.

It results therefore, that 100 parts of this acid resinous substance are formed of

Dry muriatic acid	22·5
Charcoal	35·0
Oxygen, hydrogen and carbon, in the aëriform state	42·0
	100·0.

It will be seen with surprise that the muriatic acid exists in this substance in the same proportion as in the muriate of potash; for 130 parts of this salt only contain at most 30 of muriatic acid, according to M. Thenard: but on recollecting the excellent researches of this chemist on the union of the principles of alcohol with the muriatic acid, we shall be less astonished upon seeing other hydrogenized substances act in the same manner with this acid.

The great quantity of carbon which exists in this muriated resinous matter authorizes me in thinking that it is not owing to the direct combination of the muriatic acid with the resin of camboge; for the muriatic acid when heated with this resin did not give the same acidiferous substance. It seemed much more probable to me that the resin of camboge has been partly de-hydrogenated by the oxygenated muriatic gas, and that in this state it must have contracted an union with the muriatic acid.

These results also lead me to think that during the decoloration of vegetable substances by the oxygenated muriatic acid, a part of this oxygenated acid enters into combination.

§ VII. It results from these inquiries that camboge is truly a resinous gum in every sense of the word; since we find in it a peculiar resin well characterized, and a gum resembling that which is furnished by several of our fruit-trees. It was formerly used in medicine as a remedy for gout, and hence its French name of *Gomme goutte;* but it has been disused as an anti-arthritic for nearly a century past, and is now only resorted to in order to aid the operation of other drastics: its want of taste seems to infer that its medicinal virtues are not very energetic.

ANALYSIS OF EUPHORBIA.

§ I. Euphorbia flows naturally or by incision from several plants of the same genus. In Malabar, the *Euphorbia antiquorum* still furnishes what is used by the Dutch at the present time; that which is brought into England is extracted from the *Euphorbia canariensis,* a very large species,

species, which grows to the height of 20 feet. The *Euphorbia madagascariensis* also yields a juice which the Indians thicken, and which they use, instead of our vomiting medicines, for small-pox with great success, according to Sonnerat. Other species might also yield euphorbia; but Linnæus observes that the *Euphorbia officinarum* ought to be the only species in use. Upon inspecting a great quantity of euphorbia in fine large tears, I accidentally met with some branches of the plant itself, which gave me an opportunity of ascertaining and determining the species. These branches were rather long, quadrangular, furnished with numerous tubercles ranged longitudinally, and armed at their angles with two black spears short and divergent. These characters by no means belong to the *Euphorbia officinarum*, but evidently to the *Euphorbia canariensis*, which seems to furnish the euphorbia which is brought to France.

The analytical results which the ancients obtained from euphorbia are too vague and incorrect to relate them here.

§ II. The euphorbia which I examined, was in branchy cavernous friable tears, of a pale yellow, and having the semi-transparency of wax. This substance is so acrid that, having put a small quantity on my tongue, my mouth was inflamed, with an ardent inclination to vomit immediately afterwards.

Euphorbia when exposed to a gentle heat softens easily, and loses $\frac{1}{20}$th of its weight by evaporation. I boiled four grammes of euphorbia with 100 grammes of distilled water: the filtered liquor left an insoluble matter, which when dried weighed three grammes; that which passed over was of an amber colour, and had a bitter taste accompanied with a slight acrimony.

This colour reddened turnsole tincture.

The oxalate of potash produced an abundant sediment of oxalate of lime.

The nitrate of lead forms a white sediment entirely soluble in distilled vinegar.

Lime-water also disturbs this liquor, and produces a yellow precipitate which vinegar dissolves.

§ III. A. I treated by boiling 20 grammes of euphorbia with 90 grammes of alcohol at 36°, which was sufficient for dissolving all the particles which were susceptible of solution. This solution, being filtered while in a boiling state, left on the filter a substance which was well washed with alcohol: this matter when dried weighed 6·4 grammes.

B. The alcoholic solutions were mixed, and they became turbid

turbid on cooling; and at the end of two days there was deposited an abundant white gelatinous-like matter, which when washed in alcohol and dried weighed 4·7 grammes: it still retained alcohol, which I drove off by melting it at a gentle heat; it did not then weigh more than 3·4 grammes. This substance was semi-transparent, it easily became soft under the fingers, was almost entirely volatilized on hot iron, and acted like bees-wax, of which it even had the smell when melted or burnt.

The wax of euphorbia preserved a little acrimony, without doubt because it had not been sufficiently washed in alcohol. I made a taper of it, which burned with a very pure flame.

C. The 6·4 decigrammes of the matter insoluble in alcohol A were heated to the boiling point with 100 grammes of distilled water. The filtered liquor left small branches of wood and thorns, on which the euphorbia was stuck: after desiccation it weighed 2·7 grammes.

D. The aqueous solution C when evaporated formed a varnish on the surface of the vessel. On thickening it to dryness, we obtained a fragile substance, which was removed in micaceous scales: it did not attract humidity from the air, and weighed 4·1 grammes, which I recognised at first sight as malate of lime. In short, upon heating this substance with sulphuric acid diluted in water, I obtained, 1st, some very white sulphate of lime, which after being washed and dried weighed 1·6 grammes: 2d, an acid which alcohol dissolved, and from which there were separated five decigrammes of sulphate of lime. The solution when evaporated produced two grammes of malic acid, retaining a little sulphuric acid, which was taken from it by means of barytes.

The malate of lime seems to exist therefore in a considerable quantity in the lactiform juice of euphorbia, and it is this which in the euphorbia of commerce was anciently supposed to be a gum, and which has been confounded with the extractive matter by M. Laudet, a late experimenter.

E. The alcoholic solution B, when evaporated to dryness, left a residue weighing 8·3 grammes, which were treated by cold alcohol, and which dissolved the resinous particles, and also separated four decigrammes of wax.

F. This solution was again thickened, and we obtained a resin attracting a little humidity from the air, which must have been owing to a deliquescent salt which I obtained on heating this resin with distilled water. This salt was malate of potash, and weighed four decigrammes when well dried.

G. The

G. The resin of euphorbia is of a reddish transparency, and of an excessive bitterness, which must make it be regarded as a violent poison. It becomes idio-electrical on being rubbed.

The alkalis have no sensible action on it: I heated some with a solution of caustic potash, and I poured nitric acid into the liquor, which was not disturbed: some slight red flakes only were formed after some time, which floated on the surface of the liquor.

Cold sulphuric acid dissolves the resin of euphorbia when treated with the nitric acid at a temperature of 20°. It becomes soft, yellow, and begins to be decomposed: on heating the liquor we obtain a perfect solution, which, on being evaporated, furnishes plenty of yellowish resinous-like matter, besides a soluble resino-bitter substance, and some traces of oxalic acid.

§ IV. It results from the above experiments that 100 parts of euphorbia are composed of the following substances:

Water	5·0
Wax	19·0
Ligneous matter	13·0
Malate of lime	20·5
Malate of potash	2·0
Resin	37·0
Loss	3·0
	100.

I think that the juice of the euphorbiæ of India is of the same nature with that which flows from the harmless species of Europe, which I intend to inquire into more minutely.

It is not without reason that euphorbia is regarded as one of the most powerful external cauteries and violent drastics. There is no doubt that if taken even in a small dose it would be capable of producing dangerous inflammations and erosions in the *primæ viæ*.

ANALYSIS OF MYRRH.

§ I. Myrrh, according to Lemery, is a resinous gum which issues on the incision of a thorny tree which grows in Arabia Felix, Egypt, Ethiopia, &c.

This observation concurs with the opinion of Forskal, that the balsam kafal (*Amyris Kafal* Forsk.) may be the tree which produces this gummo-resinous juice, the origin of which is so uncertain. We are inclined to think, at least,

least, that the myrrh used in commerce comes from some species of balsam trees.—Lamarck, *Encyclopédie Méthodique*, art. *Balsamier Kafal.*

The myrrh, which was the object of my researches, was in tears of varied transparency, of a reddish-yellow colour, of an aromatic taste, and slightly bitter. On breaking some of the largest pieces, we frequently remark a white nucleus marked with lines: this part, more or less opake, burns with a great flame, while the transparent tears act in the fire in the same manner as the gums. We also at times meet with almost colourless pieces, soluble in water, and having the taste of myrrh, and which are not gum arabic as has been supposed; but rather a peculiar gum, similar to that which exists in myrrh, and the properties of which I shall presently detail.

From this physical examination of myrrh it should seem that this substance is very variable in its composition.

§ II. Thirty grammes of myrrh gradually heated until the retort became red, yielded ten grammes of a brown heavy empyreumatic oil, ten grammes of a red liquid which greens the syrup of violets, yields ammonia when we mix potash with it, and acetate of potash mixed with oil if we evaporate the liquor. Hence it follows that this aqueous product is composed of ammonia in excess, acetic acid, and empyreumatic oil.

The charcoal remaining in the retort occupied less space than the myrrh employed: it was compact, irisated and brilliant, and weighed seven grammes $\frac{1}{4}$: when incinerated it produced 1·6 gramme of white ashes, which furnished with water seven centigrammes of sulphate of potash containing a small quantity of subcarbonate of potash.

That part of the ashes which was insoluble in water was entirely dissolved in nitric acid with effervescence, owing to an extrication of carbonic acid, which held a little sulphuretted hydrogen, proceeding of course from the decomposition of a small quantity of sulphate of potash by charcoal and lime.

When ammonia was poured into the nitric solution, nothing very remarkable was produced. The subcarbonate of potash separated from it all the carbonate of lime which enters singly into the composition of these lixiviated ashes.

§ III. A. Fifty grammes of myrrh distilled with water yielded a product having the smell of myrrh, and on which there floated a little volatile oil.

B. The residue of this distillation was thrown on a filter, and

and the liquor was long in passing through it. Care was taken to wash well in boiling water the substance which would not dissolve.

C. The liquor when evaporated to dryness left 23 grammes of a red transparent gum, and of a bitter taste.

1. This gum reddens turnsole tincture.

2. When treated with boiling water, it only dissolved in part, and there remained a substance of a gummy appearance, perfectly insoluble even in the weak acids. It occupied a great deal of room, became brittle after desiccation, and swelled afterwards in boiling water without being dissolved in it. This insolubility which is acquired by gum myrrh seems to be owing to heat.

3. Gum myrrh when distilled, furnished subacetate of ammonia, oil, and a charcoal, which was incinerated very easily in comparison with those of animal substances.

4. When treated by the weak nitric acid, this gum produced with the help of a gentle heat carbonic acid gas mixed with azotic gas: there was deposited a flaky yellow matter which soon disappeared: the liquid when evaporated to dryness left oxalic acid mixed with malic acid, as well as a bitter yellow matter which does not detonate.

5. The oxalate of potash produces a precipitate of oxalate of lime in the solution of gum myrrh.

6. Lime-water in excess does not affect the transparency, which indicates that it contains neither malic acid nor phosphoric acid.

7. The decoction of gall-nuts does not produce any change; and this is the case also with liquid oxygenated muriatic acid.

8. Several metallic solutions, such as those of lead, mercury, tin, precipitate this gum from its aqueous solvent: very abundant white sediments are the result.

The precipitate produced by the nitrate of lead in 23 grammes of this gum dissolved in water, weighed 20 grammes 2 decigrammes, after desiccation. It was of a reddish colour like myrrh, and was semi-transparent: boiling water did not divide it; but upon gradually adding weak sulphuric acid until there was a slight excess, the grumous particles disappeared, and it was filtered. There remained upon the filter well washed and dried 5·3 grammes of sulphate of lead containing 4 grammes of oxide, which was combined with 16 grammes of gum-myrrh, which I obtained by evaporating the liquor separated from the sulphate of lead. This gum, which held an excess of acid, did not give malic or phosphoric acid on treating it with alcohol,

hol, which inclined me to think that the lime contained in the gum-myrrh is saturated with acetic acid, perhaps also with carbonic acid; for, if we pour into the solution of this gum sulphuric acid, a slight effervescence is manifested, and a precipitate of sulphate of lime.

From the above it appears that gum-myrrh is not entirely separated from its solvent by the nitrate of lead; since of the 23 grammes, only 16 were precipitated with the oxide of lead, which could only be owing to the nitric acid set at liberty: for a salt of lead surcharged with oxide, such as the subacetate of lead, precipitates almost entirely the solution of gum-myrrh; and totally, if we add a little alkali to the mixture.

D. What remained on the filter B, when well dried and weighed, was heated with alcohol, which dissolved all the resinous parts, and lfet a soft transparent matter insoluble in boiling water and weighing six grammes after desiccation. It had all the properties of the gummy matter which I have already described.

E. The alcoholic solution D, when mixed with spirit of wine which had been used in washing the filter, gave upon evaporation 11·5 grammes of a brown resin of a bitter aromatic taste, similar to that of myrrh.

1. This resin easily softens between the fingers, melts at 48° of Reaumur, and does not become idio-electrical.

2. It gives out an aromatic smoke when burning, and yields upon distillation the same product as the resins.

3. With potash it produces a soap, the solution of which in water passes turbid through the filter.

4. Thirty-three grammes of nitric acid at 38° poured upon 5·5 grammes of myrrh-resin made it take a blackish colour: this mixture when distilled emitted some scanty red vapours. After having obtained a product of about 20 grammes of this solution, the retort was removed from the fire: it contained a supernatant resiniformed substance of an orange-colour, which when washed and dried was of a pale yellow; it weighed a gramme and a half: it was pulverulent, bitter, not very fusible, partly soluble in water, furnishing on its union with potash a soapy compound, which is very easily dissolved in water, to which it communicates a red colour without affecting its transparency. This resiniformed substance contains plenty of charcoal, and is not sensibly altered on heating it with nitric acid, which merely dissolves it. It acts in other respects precisely like that of other resins when treated by the nitric acid.

The nitric liquor (above which this substance floated) when

when evaporated to dryness left a residue, which when well washed still furnished another gramme of the same resiniform substance, which was held in solution by means of the nitric acid. Lime-water in excess, when poured into the water which had been used for washing the substance in question, separated from it 1·2 gramme of oxalate of lime mixed with a small quantity of malate of lime. The supernatant liquor contained a bitter yellow matter.

§ IV. It follows from these experiments that myrrh is composed for the most part of a gum different from any with which we are acquainted,—the chief properties of which are:

1. To acquire cohesion by means of heat, when we thicken its solutions, which renders it partly insoluble in water.

2. To produce ammonia upon distillation, and azotic gas by the nitric acid, which makes it resemble animal substances.

3. To unite with the oxides of lead, mercury and tin, by decomposing the solution of these metals.

Myrrh, besides the above, contains about 23 centiemes of its weight of a bitter resinous matter, very fusible.

ANALYSIS OF FRANKINCENSE.

§ I. I am at a loss to know on what authority Linnæus thought that frankincense was produced by the *Juniperus thurifera*, which is peculiar to Spain and Portugal, and not known in Africa or Arabia, from whence the incense is brought to us. M. Adanson, who has seen the tree that furnishes it, thinks, on the contrary, that it ought to constitute a new genus, and that it is *diœcia octandria tetragynia.* The Moors call it *soukiou.* M. Lamarck thinks this tree very like the *Amyris gileadensis* of Linnæus.

Frankincense, or olibanum, as it is met with in commerce, is a substance of a yellowish white; dry, brittle, slightly acrid and aromatic, in masses of various sizes, semitransparent, and covered externally with a white farinaceous dust produced by the friction of the tears against each other.

§ II. Frankincense melts with difficulty on the application of heat: if we set fire to it with a candle, it continues to burn by itself, and leaves a white cinder.

Twenty grammes distilled with water furnished about a gramme of volatile oil of a light citrine colour, and having the smell of citrons.

The same quantity when distilled on an open fire yields a very

a very great quantity of brown empyreumatic oil, and a little aqueous acid liquid. These products did not in any perceptible degree exhale ammonia with lime: nevertheless, on adding a little nitric acid to the mixture, slight white vapours were exhibited, which seemed to indicate the presence of volatile alkali. There remained in the retort a compact charcoal of the weight of 2·5 grammes, which left after its incineration 6·55 grammes of ashes, composed of five centigrammes of potash partly saturated by the sulphuric, muriatic, and carbonic acids, and six centigrammes of phosphate of lime; the rest was carbonate of lime, which forms the greater part of these ashes.

§ III. A. Twenty-five grammes in powder were treated with alcohol and filtered: there remained on the filter an abundant whitish matter, which, when well washed in alcohol and dried, weighed nine grammes.

B. These nine grammes of parts which were insoluble in alcohol were dissolved in boiling water, with the exception of a soft grayish substance, of a gummy appearance, and of the weight of 1·3 grammes. After its desiccation, it burned with a flame and produced a greenish resinous matter on treating it with nitric acid, which might lead us to suspect that some resin had escaped perhaps from the action of the alcohol, although I took great care to wash the residue with boiling spirits of wine.

C. The aqueous solution B, after having been filtered, produced by evaporation 7·5 grammes of a gum having the following properties.

1. This gum (which had been considered as the extractive part by the ancients, although it had no such appearance) was of a yellowish transparency, and had a faint taste.

2. It was easily dissolved in water without leaving any residue.

3. When exposed to the fire, it burnt with little flame, and left white ashes formed in a great measure of carbonate of lime.

4. Its solution in water did not redden turnsole tincture.

5. The oxalate of potash forms a precipitate in the solution of this gum.

6. The acetate of lead does not produce any perceptible change in it; but the nitrate, particularly the subacetate of lead and the nitrate of mercury, occasions very abundant thick white sediments in it, entirely soluble in distilled vinegar*.

7. The

* Schwartz, an apothecary of Jena, having prepared a mixture with the nitrate of mercury and gum arabic, observed a precipitate which Juch ascribed to the gallic acid of the gum. Van-Mons, who relates the fact, thinks

7. The decoction of gall-nuts forms a precipitate in the solution of the gum of frankincense, which is not the case with that of myrrh.

8. Lime-water in excess does not affect its transparency even after a length of time.

9. This gum is carbonized by the sulphuric acid, but without giving out acetous vapours on heating the mixture: What substance is it therefore that neutralizes the lime in this gum? I once suspected that it was benzoic acid; but this opinion requires confirmation.

10. The gum of frankincense heated with nitric acid deposits, particularly on cooling, a large quantity of a very white crystalline-like powder: on continuing the evaporation to dryness, we obtained a residue, which, after being well washed and dried, furnished the sacco-lactic acid of Scheele, (the mucous acid of Fourcroy,) forming one-third in weight of the gum employed in the experiment: the water of the washings contained oxalic and malic acid, but in a small quantity.

This action of the nitric acid on the gum of frankincense shows that it differs from gum arabic, which does not afford mucous acid.

D. The liquor produced from the 25 grammes of incense treated by alcohol A, when evaporated to dryness, afforded 14 grammes of resin; whence it results that there is a loss of two grammes, which I ascribe in a great measure to the volatile oil.

1. The resin of the incense is of a reddish-yellow colour, becomes very brittle upon cooling, has no perceptible taste, becomes idio-electrical, and resembles pitch-resin.

2. It softens in boiling water; but it requires a greater degree of heat to melt it. When exposed to a flame it burns, sending out a smell which is not disagreeable.

3. This resin when heated to dryness with a solution of caustic potash left a residue not very soluble, which, after having been well washed, is dilutable in boiling water, producing an emulsive liquid more or less thick. The resin of pitch unites with the alkalis with much more facility *.

4. Cold

thinks, on the contrary, that it is owing to a decomposition of the nitric acid and of the metallic oxide: but the examination of these sediments constantly presented to me a combination of gum and metallic oxide.

Gum arabic is also abundantly precipitated from its solutions by the subacetate of lead: it is the same case with gum adracanth. These sediments resemble cheese: if we burn them, they leave lead in the metallic state.

* Resinous substances have not been as yet examined with the minuteness and attention which they deserved, as may be easily proved, from the few and

4. Cold sulphuric acid, the resin of incense, and water produce a white sediment in this solution, which is of a red colour. If we heat it for some time, the water then separates from it a black resin soluble in the nitric acid, which converts it by evaporation into a brown residue of an astringent bitter taste, and the solution of which in water precipitates gelatine, but not sulphate of iron.

5. The resin of incense when heated to dryness with eight times its weight of nitric acid at 38°, is converted almost entirely into the resiniform matter, the properties of which I have already described. The washings which did not contain oxalic acid furnished by evaporation a residue, which when again heated with nitric acid did not give the tannin substance discovered by Mr. Hatchett, but a bitter soluble substance, which was partly precipitated upon cooling from its solution in boiling water, soluble also in alcohol, and producing precipitates in several metallic solutions.

This substance seems to me to have properties analogous

and frequently inaccurate observations which have been made as to the chemical properties of these bodies. Thus it has been generally supposed that the alkalis exercise no action on the resins. In order to verify this assertion, I made the following experiments on the resins:

To a very weak solution of potash, cold, I added gradually some pitch-resin in powder: there resulted a perfect saponaceous solution, which became thick and thready like the white of an egg. The weakest acids produce very abundant white sediments: it is the same with the alkalis and the neutral salts, on account of their greater affinity for water than that of the resinous soaps. A large quantity of common water also decomposes this soap; the sediment which is the result is only partly soluble in alcohol: that part which cannot be dissolved still contains plenty of resin united to lime.

If we pour muriate of lime into the solution of this resinous soap, the mixture goes into a whitish mass resembling broth, and formed of resin and lime.

All the metallic solutions decompose *in toto* the soap of pitch-resin. The sediments which are produced might in many cases be employed in painting.

Fifty grammes of pitch-resin heated until saturation with solution of potash were entirely dissolved: we obtained by evaporation 69 grammes of solid resinous soap, dry at a cold temperature, of a brown colour, and which I found to be perfectly similar to Starkey's soap. Messrs. Baumé and Legendre had therefore good reason to think that it is only the thick and resinous part of the essence of turpentine which can be really combined with potash: for this alkali acts upon volatile oil, only by favouring its conversion into resin in order to unite with it afterwards.

The volatile alkali greatly diluted in water, also unites with pitch-resin with great facility, and speedily converts it to the soapy state. This combination diluted with water acquires through time the consistence of tar.

I ought not to omit to mention, that the soap made from potash and the above resin might be used instead of common soap for domestic purposes. It produces abundance of froth, and whitens well: only it gives a slight resinous smell to linen, which is dissipated, however, on exposure to the air. The œconomy and facility with which it might be manufactured would doubtless enhance its properties.

with

with that which I have designated by the name of *resino-bitter*, but it differs from it in the stability and proportion of its elements.

ANALYSIS OF GUM AMMONIA.

§ I. No traveller has ever described the plant which produces gum ammonia; but from the inspection of the seeds which are always found in the leaves of ammonia, it is at least very probable that it flows in a milky form from one of the umbelliferæ, particularly when we recollect that other gummo-resins also flow from the juice of several feru-laceous plants. In fact, the seeds which we meet with in the gum ammonia are oval, compressed, relieved on each side by three longitudinal striæ, and composed of two elliptical seeds laid against each other: these characters belong, as we may be satisfied, to the genus Ferula. Hence it seems to me to result that a species of this genus furnishes the gum ammonia which is brought from the deserts of Africa, and from Libya to Alexandria, from whence it reaches us as an article of commerce.

The gum ammonia which I made use of in my experiments was in irregular masses, yellowish externally, slightly transparent on the edges when they are thin, presenting a shell-like fracture, shining, white, sometimes slightly marbled, and having the aspect of certain resiniform silices, particularly the variety known by the name of *cacholong*. It has little smell, at least if it be not pulverized. Its taste is slightly acrid, bitter, and nauseous. It is easily diluted in water, producing a very white milky liquor.

It should seem that the ancients have rather ventured upon conjectures as to the nature of the substance in question, than made any real experiments: thus they say, that boiling water dissolves it almost entirely, which made Cartheuser suspect that the extractive part is more abundant in it than the resinous part; but we shall see that this assertion is unfounded.

§ II. Gum ammonia when exposed to a heat incapable of decomposing it easily becomes soft, and loses $\frac{6}{100}$ of its weight in humidity.

Twenty-five grammes of this substance furnished on being distilled on the open fire twelve grammes of liquid, the greater part of which was a brown empyreumatic oil: this product when mixed with lime set free a smell of volatile alkali. There remained in the retort a charcoal weighing 7·5 grammes, which furnished after its incineration 3·2 grammes of ashes mixed with gravel, which furnished to

 the

the weak nitric acid 1·5 decigramme of phosphate of lime, which was separated from it by ammonia, in addition to 3 decigrammes of carbonated lime, separated by the sub-carbonate of potash.

§ III. A. I boiled 25 grammes of gum ammonia in powder with a sufficient quantity of alcohol at 36°. The liquor being filtered while boiling deposited nothing in cooling: there remained on the filter a white substance, which when dried, and after being washed in alcohol, weighed 5·8 grammes.

B. This substance when heated with water was in a great measure dissolved in it: the liquor when passed through a fine cloth left a grayish, soft, glutinous substance insoluble in water and in spirits of wine, of a black colour after its desiccation, and of the weight of 1·1 gramme. It burns more easily than the gums, produces a grayish cinder, and affords a yellow colour with a little oxalic acid on treating it by the nitric acid.

C. The aqueous solution B when evaporated furnished 4·6 grammes of gum.

1. This gum is transparent, of a reddish-yellow colour, and of a slightly bitter taste; it is tolerably brittle, and is easily dried in comparison with the other gums.

2. It burns without any apparent flame, producing a white cinder which is dissolved with effervescence in the acids.

3. Water dissolves it entirely: this solution scarcely reddens turnsole tincture.

4. It is precipitated in abundance and entirely from its solutions by the subacetate of lead, (but not with the acetate,) by the nitrate of lead and mercury, only the latter renders the liquor milky.

5. Lime-water in excess does not affect the solution of this gum; it does not therefore contain phosphoric acid, but its ashes give a little phosphate: hence it seems to me to result that phosphorus exists in this gum in a state of combination with the other elements which constitute it.

6. The oxalate of ammonia produces in its solution a precipitate of oxalate of lime, but I am ignorant of the acid which neutralizes this lime in the gum*.

7. The solution of gall-nuts does not manifest any thing remarkable.

8. When treated by the nitric acid of commerce, this

* M. Vauquelin, who recognised the phosphate of lime in the ashes of gum arabic and gum tragacanth, thinks that the lime is saturated in a great measure in these gums by the acetic acid.

gum

gum gave nearly the same results with frankincense: *i.e.* a great quantity of mucous acid, oxalic acid, and very little malic acid.

D. The alcoholic solution A when inspissated to dryness furnished 17·5 grammes of a resin, which exhibited the following characters:

1. It is of a reddish-yellow, transparent, and brittle like wax at a cold temperature, having an undulating and glossy fracture, slightly receiving the impression of the nail, and easily becoming soft in the mouth or between the fingers. It has no remarkable taste. It smells like gum ammonia. It does not become idio-electrical.

2. Like wax, this resin melts at a temperature of 43° of the thermometer. At a higher temperature it swells considerably, gives out a particular smell, and leaves a spongy light charcoal occupying a good deal of room.

3. This resin unites easily with the alkalis, even when cold, and soapy solutions of considerable bitterness are produced *.

4. The sulphuric acid dissolves with facility the resin of gum ammonia, particularly at a gentle heat. Water decomposes this solution: if we heat it more, there results a hydrogenated charcoal, which after having been washed was dissolved in the nitric acid, and furnished by evaporation an astringent substance which precipitates glue in brown.

5. The nitric acid when heated on the resin of gum ammonia, at first only gave some white vapours; but suddenly these nitric vapours appeared with great vehemence, and there was produced a yellow resiniform substance, which was dissolved during the operation. On inspissating the liquor I obtained a resinous-bitter substance of a very pure yellow, fusible at a gentle heat, uniting with the alkalis, soluble in alcohol and in boiling water, and depositing a little therefrom upon cooling; partly soluble also in a great quantity of cold water, and communicating to its solutions a very beautiful yellow, which adheres very strongly to the fingers, and may be fixed with great facility to silk and woollen, giving them a superb lustre, unalterable by the oxygenated muriatic acid, and even by the weak alkalis: in solidity therefore it seems to be far superior to any dye-stuff we are acquainted with. I may even add, that so far as œconomy is concerned, this fine colour seems to merit a

* The soap of pitch-resin is also very bitter to the taste; which seems to bespeak very energetic properties as existing in these resins, which ought therefore to fix the attention of medical practitioners.

preference over other yellows, on account of the small quantity of it which is requisite.

When mixed with gum, this substance might give a much purer and much more solid yellow than camboge.

From this examination of gum ammonia, it results that 100 parts are composed as follows:

Gum	18·4
Resin	70·0
Glutinous matter	4·4
Water	6·0
Loss	1 2
Extractive matter	0·0
Total	100.

LXI. *Explanation of an Experiment on Tuning, in Reply to a Correspondent. By* M.

Sir, It was not till late in April that I had the pleasure of receiving your Number for the preceding month, otherwise I should have sent you the following remarks at an earlier period.

If the author* of art. xxxii. will, uninfluenced by passion, re-examine my letter on modes of tuning, (vol. xxxvii. p. 111), he will not discover any expressions to justify the charge of my having asserted the accuracy and success of a hasty experiment, there faithfully described; of my having given any estimate of the power of any description of monochord; nor of my having made a boast of any skill of my own: after due consideration, it will therefore appear that, instead of opposing my assertions, instead of controverting opinions which I have stated, this discerning writer has, in the elegant article referred to, been combating the mere creatures of his own disturbed imagination. We at the conclusion of the experiment, were, I think, as fully sensible as that very ingenious writer may be, of its errors; and I should have inserted a table of the differences between the scale of sounds produced and the system attempted, had I been disposed to waste your valuable pages on unnecessary schoolboy-calculation. I repeat, that with practice, and by employing longer time, the errors would be

* I shall consider the Rev. C. J. Smyth to be the author, till I am better informed. Perhaps the person who could write it, will not be ashamed to avow himself the author.

less;

less; if it were necessary to have recourse to such a method. But, although I have said that the majority of practical tuners are not guided by calculation, and that therefore such persons are not completely qualified to determine which is the best of the numerous unequal temperaments already published; I have nowhere affirmed that they tune by the melody. Nor have I ever imagined that the *differences* of monochord-lengths are proper measures of the intervals. This person boldly asserts, but for what purpose I cannot discover, that "tuners *never* resort to the melody in tuning." How then, let me ask him, is a tuner enabled to perceive whether the "conchord" which he is tempering be a fifth or a fourth? It is probable that his answer will not be hostile to what I have before advanced. I once saw a professional tuner much embarrassed in a first attempt to adjust the *bi-equal* thirds* of the Stanhope temperament; not perceiving, till he had recourse to the melody, that one of his thirds was beating flat.

It is with most tuners by profession as with the composer Grétry; who, after declaring himself ignorant of calculation, and describing his method of tuning by thirds, by fourths, fifths, sixths, and octaves, adds, "C'est alors un tempérament de sentiment qui guide l'oreille, et non celui de calcul." (Essais, vol. ii. p. 362). Their rule is the same as Keller's; namely, to make all the IIIds as sharp, and all the Vths as flat, as the ear will permit.—See Holder's Harmony, 8vo, 1731; and Catalisano's Grammatica Armonica, 1781, p. 78 and p. 156.

In my last communication, F·750 instead of F·749, is an error of transcription. The following asterisk should have referred (as well as the first) to Chladni's Acoustics, wherein the lengths for the equal temperament are: C1.00000, C♯ or D♭·94387, D89090, D♯84090, E79370, F74915, F♯70710, G66742, G♯62996, A59461, A♯56123, B52973, C50000, p. 37, § 26.

The monochord we employed was constructed to determine the lengths of wire more minutely than your correspondent seems to have supposed; but owing to an accident, it could not be depended on to more than three places of decimals: three were therefore preferred to more, having only the semblance of greater accuracy. From this, he very logically concluded that we were quite ignorant of his seeming profundities.

To conclude, I leave Mr. Smyth to the arrangement, as

* See Mr. Farey's article "Bi-equal," in the Edinburgh Encyclop. vol. iii. p. 497. (1811.)

to the enjoyment of his feast of *palatable* dishes, as long as he is without the power of obliging me to conform to his *taste;* but expecting that, when he publicly holds up one system as better than others, he should give sufficient reasons for the preference: and I assure your correspondent, that whatever title he may next apply to himself, or to his opponent, will to me be a matter of the purest indifference.

May 1, 1811. A. MERRICK.

LXII. *Method of procuring Turpentine and other Products from the Scotch Fir. By Mr.* H. B. WAY, *of Bridport Harbour**.

SIR, THE enormous high price of turpentine, tar and pitch, last year, brought to my remembrance that I had, in 1792, when in America, made some memorandums on the subject of obtaining them in North Carolina, which, on referring to, led me to think that they might be obtained in this country. I was induced to mention it to my relation and friend, John Herbert Brown, Esq. of Weymouth, and of Sheen, in Middlesex, when on a visit at my house, and I expressed a wish that I could try the experiment with regard to turpentine; when he very kindly gave me leave to try it on three trees growing on his estate, about three or four miles from this place, and he went with me and fixed on them, and early in last April I had them prepared for the purpose of extracting the turpentine, and they have been running till the 18th instant. The weather, except the last month and part of this, has, from so much rain falling, and there being so little hot weather, been particularly unfavourable for this business, as, the distance being such as to prevent the trees being regularly attended, the hollows were frequently found by my men full of water, and a good deal of the turpentine, which ran off with the water, lay on the ground. Under all these circumstances I was only able to obtain from the three trees about two pounds and a half of turpentine. Mr. Brown being with me again the 16th and 17th instant, as he wished to take the trees down, I begged he would allow me to take a part from one of them, for the purpose of sending to the Society of Arts, Manufactures, and Commerce, with the turpentine collected from the trees; which he most readily complied with.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* vol. xxviii.——The Society voted the silver medal to Mr. Way for this communication.

with. I have therefore taken about six feet from one of them, (they were all nearly the same size); what I have sent is the part from the ground to the top of the place that has been cut away for the turpentine to run into the hollow, from whence it was to be collected; the hollow was cut in this considerably higher than is usual in America, as this tree stood in a hedge, and could not well be hollowed lower; I have matted up this part of the tree, and secured it with straw and a double mat, to prevent the bark being rubbed off, that it may be seen in the same state as it stood when the turpentine was taken from it; the turpentine is in the cask in which it was deposited when brought from the trees, and I have this day shipped both on board the sloop Betsey, Captain Trent, bound to Downe's Wharf, London, directed to you, freight paid here by me, which vessel I expect will sail in a day or two, and I hope you will receive them safe; which when you do, you will much oblige me by requesting that both may be examined, in the hope that this small trial may meet with the approbation of the very highly respectable and truly useful Society of Arts, Manufactures, and Commerce; and if considered likely to prove useful, that they may induce some person who has the means and opportunity of doing it, to make a trial on a larger scale, so as to fairly ascertain whether turpentine can be obtained in this country from the very large and numerous plantations of Scotch firs, now in the United Kingdom, previous to the trees being cut down, either to thin plantations, or where ground is designed to be cleared, as taking the turpentine from the trees previous to their being cut does not at all injure the wood, and by making the hollow in the trunk of the tree about six inches from the ground, it would waste but a very small quantity of timber. I have taken the liberty of annexing a copy of memorandums I made when in North Carolina, respecting the modes of collecting turpentine, and making tar and pitch, in hopes they may afford the Society some little information, as they are not, I apprehend, very generally known. They are copied from memorandums which I actually made on the spot. I would have sent the memorandum books with this, had not the remarks been mingled with others relative to my commercial pursuits; but I shall have no hesitation in allowing any person to examine them, or to afford any information in my power to any persons willing to make experiments in this way, if they will favour me with a call. I am well satisfied in my own mind, that very large quantities of tar might be obtained from the knots and limbs of the

the Scotch fir when cut down, and that the charcoal made from it would not be injured by the tar being first extracted; and as I was in Norway, Sweden, and Russia, in 1789 and 1790, and saw no tree from which I consider that tar could be extracted, except the Scotch fir, or red deal, which is one and the same tree, I am persuaded that the refuse of that tree must be what they make the tar from in those countries, though I had no opportunity of seeing the process there. I suspect that the Swedish tar-kilns must be constructed of brick, or some sort of masonry, as the tar from thence is much clearer, better, and more free from extraneous matters than that of any other country. I have observed the tar from North Carolina to have frequently a quantity of sand in it, which is easily accounted for, from the soil in which the kilns are made; it would, in the careless way in which they take it out of the hole dug in a sandy soil, be very likely to be mixed with the sand. In the small cask, in which the turpentine is, I have sent a few small red deal knots from some timber that I have lately taken out of my warehouse, on some alterations being made; the timber from which they are taken has been in the warehouse ever since the summer of 1786, and yet when these pieces are exposed to a moderate heat, the tar will be seen to exude from them.

I remain, sir,

Your obedient and very humble servant,

Bridport Harbour, Nov. 27, 1809. H. B. WAY.

To C. Taylor, M.D. Sec.

Extracts of Notes taken by Mr. Way.

Thursday, April 12, 1792.

Arrived at Wilmington, North Carolina, about one P.M. Observed on the roads the pitch-pines prepared for extracting turpentine, which is done by cutting a hollow in the tree about six inches from the ground, and then taking the bark off from a space of about 18 inches above it, from the sappy wood. The turpentine runs from April to October, and is caught by the hollow below. Some of the trees were cut on two sides, and only a strip of the bark left of about four inches in breadth on each of the other two sides, for conveyance of the sap necessary for the support of the tree. A captain Cook, with whom I had been travelling, informed me that some trees would run six or seven years, and that every year the bark was cut away higher and higher, till the tree would run no longer; and I observed many that had

had done running, and they were in general stripped of the bark on two sides, as high as a man could reach, and some were dead from the operation; others did not look much the worse for it. I find the usual task is for one man to attend three thousand trees, which taken together would produce one hundred to one hundred and ten barrels of turpentine.

April 15, 1792.

On my return from Wilmington to Cowen's tavern, distant about sixteen miles from thence, I was informed that the master of the house had been a superintendant of negroes who collected turpentine. I found the information I had before received was not perfectly correct; he told me he attended to six slaves for a year for a planter, and between the 1st of April and the 1st of September they made six hundred barrels of turpentine. The cutting the trees for the purpose of collecting is called boxing them, and it is reckoned a good day's work to box sixty in a day; the trees will not run longer than four years, and it is necessary to take off a thin piece of the wood about once a week, and also as often as it rains, as that stops the trees running. While in North Carolina, I was particular in my inquiries respecting the making tar and pitch, and I saw several tar-kilns; they have two sorts of wood that they make it from, both of which are the pitch-pine; the sort from which most of it is made are old trees which have fallen down in the woods, and the sap rotted off, and is what they call light-wood, not from the weight of it, as it is very heavy, but from its combustible nature, as it will light with a candle, and a piece of it thrown into the fire will give light enough to read and write by; all the pitch-pine will not become light-wood; the people concerned in making tar know it from the appearance of the turpentine in the grain of the wood: the other sort of wood which is used, after the trees which have been boxed for turpentine have done running, they split off the faces over which the turpentine has run, and of this wood is made what is called green tar, being made from green wood instead of dry. When a sufficient quantity of wood is got together, the first step is to fix a stake in the ground, to which they fasten a string, and from the stake, as a centre, they describe a circle on the ground according to the size they wish to have the kiln; they consider that one twenty feet in diameter and fourteen feet high should produce them two hundred barrels of tar; they then dig out all the earth a spit deep, shelving inwards within the circle, and sloping to the centre; the earth taken out is thrown up

up in a bank about one foot and half high round the edge of the circle; they next get a pine that will split straight, of a sufficient length to reach from the centre of the circle some way beyond the bank; this pine is split through the middle, and both parts are then hollowed out; after which they are put together, and sunk in such a way, that one end which is placed in the centre of the circle is higher than that end which comes without the bank, where a hole is dug in the ground for the tar to run into, and whence the tar is taken up and barrelled as it runs from the kiln. After the kiln is marked out, they bring the wood, ready split up, in small billets, rather smaller than are generally used for the fires in England, and it is then packed as close as possible, with the end inwards, sloping towards the middle, and the middle is filled up with small wood and the knots of trees, which last have more tar in them than any other part of the wood; the kiln is built in such a way, that at twelve or fourteen feet high it will overhang two or three feet, and it appears quite compact and solid. After the whole of the wood is piled on, they get a parcel of small logs, and then place a line of turf, then another line of logs, and so on alternately all the way up, and the top they cover with two or three thicknesses of turf. After the whole is covered in this way, they take out a turf in ten or a dozen different places round the top, at each of which they light it, and it then burns downwards till the whole of the tar is melted out; and if it burns too fast they stop some of the holes, and if not fast enough they open others, all of which the tar-burner, from practice, is able to judge of. When it begins to run slow, if it is near where charcoal is wanted, they fill up all the holes, and watch it to prevent the fire breaking out any where till the whole is charred; the charcoal is worth two-pence to three-pence, British sterling, per bushel. It will take six or eight days to burn a tar-kiln; in some places they burn it at such a distance from the shipping that they have very far to roll it, and even then sell it at from 3s. 6d. to 5s. British sterling, per barrel, sometimes taking the whole out in goods, but never less than half the amount in goods; from all which it will be reasonably supposed that tar-burning in that country is but a bad trade, as it must be a good hand to make more than at the rate of a barrel a day; the barrels cost the burner about 1s. 3d. British sterling, each; the tar-makers are in general very poor, except here and there one that has an opportunity of making it near the water-side. Pitch is made by either boiling the tar till it comes to a proper thickness, or else

else by burning it; the latter is done by digging a hole in the ground, and lining it with brick; it is then filled with tar, and they set fire to it, and allow it to burn till they judge it has burnt enough, which is known by dipping a stick into it, and letting it cool; when burnt enough they put a cover over it, which stops it close, and puts out the fire. Five barrels of green tar will make two of pitch; and it will take two barrels of other tar to make one of pitch.

N. B.—The foregoing observations respecting tar and pitch, are copied from a memorandum made by me at Suffolk, in Virginia, on the borders of North Carolina, April 23, 1792, and are the result of the inquiries and observations I made on the subject whilst in Carolina.

Wilmington, N. C. April 13, 1792.

In conversation with a Mr. Hogg, who had been settled there and at Fayette-ville before the war, I learnt that pitch-pine timber growing on the sands was the best, and that it was reckoned to be better if cut in the winter before the sap rises in the tree.

H. B. Way.

Sir,—It affords me much pleasure to learn that my communication, on the extraction of turpentine from the Scotch fir, has been thought worthy of the consideration of the Society, and it will be highly gratifying to me, if it should induce persons who have considerable plantations to try it on such a scale as to ascertain to what extent it might prove beneficial in this country. The experiment should be tried on trees so situated as to be conveniently examined every day, and the turpentine collected in the hollows removed as often as possible to prevent its being injured, or wasted by the rain. I think, that during the American war, some importations of turpentine were made from Russia and Sweden, and if so, it must have been extracted from what we call the Scotch fir in a colder climate than this. The article called Venice turpentine, which is brought from Carinthia and Carniola, is extracted there from the larch tree; and it might probably answer to try to produce it from the larch trees grown in Great Britain, in the same way as I have collected the turpentine from the Scotch fir. Respecting the wood of the Scotch fir being injured by the extraction of the turpentine from it, I should rather think that it would, on the contrary, be the better for it; as all those who use deals from Scotch fir, in this neighbourhood, complain that it is too full of turpentine to work well. The fact might be ascertained, by the piece of timber which I sent

sent to the Society, as, if it was wished to preserve that part in which the hollow is made, the back part, or nearly half of the tree might be sawn into boards without injury, and those boards might be compared with some from a tree taken down in the winter, from whence the turpentine has not been extracted: it must however be noted, that from the tree I have sent to the Society, the turpentine has only been running one year, whereas, in America, they collect the turpentine from the same tree for three or four succeeding years. It has been supposed and asserted that turpentine was only obtainable from the United States; but I have sufficient documents to prove, if required, that a very large quantity of it can be procured from East Florida; and I well remember, that about the year 1782 several cargoes of turpentine were shipped in the river St. John's, for Britain; and though that country is at present in the hands of the Spaniards, no doubt, arrangements might be made with the Spanish government for a supply of that necessary article from thence. It is my earnest wish, that through the medium of the Society of Arts I may render any information that may be serviceable to the interest of the united empire, and I will, with pleasure, furnish further communication on the products of Florida and its commerce, if desired by the Society.

I am convinced that tar might be produced from the refuse of firs of English growth to advantage, and that a much better article could be made from them in Britain, than any imported from America. The Scotch firs, in England, from being planted at greater distances from each other than they are naturally found abroad, have much larger knots, and greater numbers of them, than in Carolina, or the North of Europe, and would therefore produce more tar, in proportion, from their refuse wood than the trees of those countries.

The pitch-pines of Virginia, the Carolinas, Georgia, and the Floridas, grow to an immense size in what are there called pine-barrens, the soil of which is finer and whiter than the sand used as writing-sand in Great Britain, and the trees grow almost to the verge of high-water mark on the sea shores. I think it would answer a good purpose for the Society to encourage, by premiums, the extraction of turpentine from British firs.

I remain, sir,

Your obedient and very humble servant,

Bridport Harbour, April 21, 1810. H. B. Way.

To C. Taylor, M.D. Sec.

Re-

Reference to the Description of Mr. H. B. Way's *Method of procuring Turpentine from Fir Trees. Pl.* VIII, *Fig.* 4.

a, Represents the lower part of a fir tree, as growing in the earth; *b*, shows the part where a portion of the bark is taken off to assist the emission of the turpentine; *c*, is a hollow cut within the body of the tree, it is in the form of a bason at the lower part to receive the turpentine, which exsudes into it from the pores of the tree: this bason is about six inches from the ground.

LXIII. *On the Strata of Mountains.* *By* William Richardson, *D. D.*

To Mr. Tilloch.

Sir, Between two and three years ago, a friend transmitted to me from London a Number of your Philosophical Magazine, containing a paper from Mr. Farey, in which he is pleased to speak in most flattering terms of a memoir of mine, published in the Philosophical Transactions for the year 1808.

The subject was the arrangement of the strata in the country where I lived, with the entire removals of vast portions of them, showing the terminations of the strata left behind, often abrupt, and sometimes forming a perpendicular façade, where the abrupting agent acted on an accumulation of strata.

Mr. Farey exults that a gentleman who had never met with his observations on the strata of England, nor read his publications on their subject, should find that the strata in Ireland had the same operations performed upon them; sometimes partial abruptions, sometimes entire removal of vast portions of these strata, without leaving a trace behind to aid our conjectures in discovering what was become of such vast portions of matter.

Mr. Farey speculates upon the agent that performed these mighty operations: I do not presume to venture so far. I limit myself to *facts*, from which those who choose to avail themselves of my data, may draw such inferences as they think can be sustained.

Cosmogonists, who by their respective theories pretend to lay open original formation, carry their views too far back, and overlook a most important step in the discussion, that is, the actual arrangement of our materials, however originally formed.

Enu-

Enumeration of the strata of which any country is composed, with an account of the changes they have undergone, and the operations that have been performed upon them, forms the true geological history of that country, and an accumulation of many of these detached histories affords the best materials for a general history of the physical world.

Statistical surveys seem the proper deposits for the curious facts of every country.

Mr. Farey avails himself of this opportunity to lay before the world the stratification of *Derbyshire;* and in my contribution to my friend Mr. DUBOURDIEU's Statistical Survey of *Antrim,* I shall trace the arrangement of the strata of that *county* with much accuracy.

The curious basaltic construction of the Giant's Causeway in my neighbourhood, first brought my attention on such subjects.

From the singular *forms* which nature had impressed upon her materials in that wonderful spot, I proceeded to her arrangements, the source of the beauty and grandeur of our coast, displayed in a succession of magnificent façades.

The consummate regularity of these arrangements I laid before the world in the memoir I have mentioned, and gave a detailed account of the operations that had been performed on them.

I now proceed from the more diminutive arrangements of nature, so well exhibited in our magnificent façades, to her grander arrangements, the construction of her great mountains, all composed (with us) of vast strata.

These strata it appears are abrupted at the periphery; and the materials carriedoff; *always* at one side; and sometimes in the whole contour, as in the great hemispherical mountain KNOCKLAID, whose middle frustum is a vast stratum of white limestone, showing itself every where round the periphery, several hundred feet high, forming a steady plane, slightly inclined to the horizon: which plane so far above the surface, *on the mountain,* is found, when traced in the direction of its dip, again to catch the surface of the earth, in its rectilineal course, at a few miles distance.

Can it be sustained for a moment, that *Knocklaid* was originally formed as it now stands, bold, and solitary? Is it not rather a portion of the great consolidated mass of strata left standing when the materials once contiguous were carried off from its whole contour?

The

The particular arrangements of other *Antrim* mountains, and their local circumstances, lead us to the very same conclusion.

Were we from these data to *generalize*, and, calling in the aid of *analogy*, to pronounce *all* mountains to be similar to those of *Antrim*, must we not form the general conclusion,

That the mountains of the world were not *formed*, but *left* behind,—not the stupendous constructions of mighty agents,—but the scattered remnants of a diminished world?

Moy, Ireland, May 12, 1811. W. RICHARDSON, D.D.

LXIV. *Some Remarks on the Physiology of the Egg: communicated in a Letter from* JOHN AYRTON PARIS, *M.B. to* WILLIAM GEORGE MATON, *M.D. V.P.L.S. &c. &c.**

DEAR SIR, THE extensive range which the *ovipari* form in the scale of animated existence renders the physiology of the egg a subject of extraordinary interest and improtance to the disciple of Linnæus: I am therefore induced to hope that the communication of any new facts relative to its organization and development will be received by you as an acceptable tribute to the cause of natural history.

The *ova*, or germs of oviparous animals, admit of an evident division into two orders. I. The Perfect, and II. the Imperfect. The former are deposited by the *aves*, *serpentes*, and by most *oviparous quadrupeds*, and are completely formed *in utero;* whilst the latter, produced by some of the *testacea*, *amphibia*, and by most *pisces*, acquire additions after their exclusion. The observations contained in this memoir relate more particularly to the class *aves*, the history of whose *ova* comprehends whatever is interesting or important in the germs of inferior animals. The egg, when completed and deposited, consists of the following parts:

1. *Vitellus* or *yolk*, with its *capsule* and *cicatricula;* 2. The two *albumina*, with their proper membranes; 3. The *chalazæ;* 4. The *folliculus aëris;* 5. The *common membranes;* 6. The *exterior involucrum*, or *shell*.

The necessity of any description of these parts is superseded by the minute and valuable details which are to be found in the works of Fabricius ab Aquapendente, Harvey, Malpighi, and of many modern and enlightened physiologists;

* From the Linnean Transactions, vol. x.

logists; I shall confine myself, therefore, to what I consider exclusively original.

The principal use of the albuminous portion of the egg is doubtless to afford materials for the growth, and nourishment for the support, of the ovular embryon: such however does not appear to be the *only* purpose for which it is designed. Nowhere does Nature display more anxiety for the preservation of her offspring, or more wisdom to obtain her objects, than in her provisions to ensure an equable temperature to the *fœtus in ovo*: a condition which is so essential to the evolution of the animal, that the smallest deviation overthrows the nice balance between the different actions that are to mature it, and produces fatal effects. The *albumen* then I consider as a great defence against such an evil. The *chalaza*, by retaining the *cicatricula* at the source of heat, obviates the mischief that would accrue from constant change of position; but the *albumen*, being a most feeble conductor of caloric, retards the escape of heat, prevents any sudden transition of temperature, and thus averts the fatal chills which the occasional migrations of the parent might induce. As an illustration of the use and importance of such a structure, I may observe, that those fish which retain their vitality a considerable time after their removal from the water, as eels and tench, have the power of secreting a slimy and viscid fluid, with which they envelop their bodies. Is it not extremely probable that this matter, by acting like the albumen of the egg, and preventing evaporation from the surface of the animal, and the consequent change of temperature, may be the principal cause of this tenacity of life?

It must however be remarked, that deviations of temperature are injurious and fatal in proportion only to the degree of vital energy which the ovular embryon possesses: hence germs of inferior vitality not only suffer the vicissitudes of heat and cold with impunity, but are developed by a less defined temperature. We therefore perceive, as we descend the scale of oviparous beings, that those peculiar provisions which the eggs of perfect animals possess, for the regulation of their temperature, cease to be essential, and therefore disappear.

The part of the egg to which I next beg to direct your attention is the *folliculus aëris*, or air-bag, placed at its obtuse extremity; the nature of this follicle excited in me considerable interest, as I found that it had not been so fully investigated as its importance seemed to demand.

The external shell, and the internal membrane by which it

it is lined, constitute the *parietes* of the cavity, whose extent in the recent egg scarcely exceeds in size the eye of a small bird: by incubation, however, it is extended to a considerable magnitude. That its most essential use is to oxygenate the blood of the chick, in my opinion there can be no doubt: but to establish completely the truth of such a theory, it is necessary to discover the nature of the air by which it is inflated, and which has hitherto remained unexamined. We are informed by Buffon, that it is a product of the fermentation which the different parts of the egg undergo. If the count's conjecture be established, it must be non-respirable, and therefore cannot discharge the office which such a theory would assign to it. To determine this matter, and to discover also whether the process of incubation produces any change in its chemical constitution, I instituted the following experiments; viz.

Experiment I.—Twenty-one hen's eggs newly laid, when punctured at their obtuse extremity, yielded only one cubical inch of gas, which, when received in a jar, and subjected to the eudiometric test of Dr. Priestley, I found to be pure atmospherical air.

Experiment II.—Two eggs, after 20 days' incubation, were opened under the surface of water, from which one cubical inch of gas was collected: this I also discovered to be atmospherical air, contaminated however with a small portion of carbonic acid, which I suspect to be derived from the venous blood of the chick, and which seems to establish another most beautiful analogy between this mode of oxygenation, and respiration after birth.

From these results the following corollaries may be drawn: viz.

1. The *folliculus aëris* before incubation contains atmospherical air.

2. No other chemical change takes place in the constitution of the air, than a small inquination with carbonic acid.

3. It gains by incubation an increase of volume, which takes place nearly in the ratio of ten to one.

I must here remark, that its extent does not increase equally in equal successive portions of time, but observes a rate of progression, which is accelerated as the latter stages of incubation advance: it seems, however, to arrive at its *maximum* of dilatation a few days previous to the exclusion of the animal.

In the eggs of inferior animals, the embryon does not appear to be oxygenated by any distinct apparatus, but, like the animal which it is hereafter to become, receives air

through the medium of *spiracula*, dispersed over the exterior *involucrum*. The description of the *folliculus aëris* just delivered is taken from that in the egg of our common hen. The same apparatus exists in the eggs of all birds, and contains a similar air: its capacity, however, does not seem to vary either with the size of the egg, or of the bird to which it belongs; but I think I have discovered a beautiful law by which its extent is modified.

I have uniformly found, as far as my contracted inquiries have led me, that the *folliculus aëris* is of greater magnitude in the eggs of those birds which place their nests on the ground, and whose young are hatched fledged, and capable of exerting their muscles as soon as they burst from their shell, than in the eggs of those whose nests are generally built on trees, and whose progeny are born blind and forlorn. Thus the *folliculi* in the eggs of fowls, partridges, and moor-hens are of considerable extent, whilst those in the eggs of crows, sparrows, and doves are extremely contracted. The chick, therefore, of fowls and partridges has a more perfect plumage, and a greater aptitude to locomotion, than the callow nestlings of doves and sparrows. Such an instance of the agency of oxygenation in the promotion and increase of muscular power is not solitary in physiology; for the history of ruminating animals will furnish us with a parallel example. "Their cotyledons," observes the author of *Zoonomia*, "seem to be designed for the purpose of expanding a greater surface for the termination of the *placental* vessels, in order to receive oxygenation from the *uterine* ones: thus the progeny of this class of animals are more completely formed before their nativity than that of the carnivorous classes. Calves, therefore and lambs can walk about in a few minutes after their birth; while kittens and puppies remain many days without opening their eyes." If any further testimony be necessary to show that the augmentation of muscular energy is the result of a nice combination of oxygen with the animal organs, many interesting facts might be adduced in confirmation of its truth. We generally find the strength of an animal proportionate to the extent of its chest: hence an attention to the "*animosum pectus*" has been attended with the improvement of our breed of cattle; and it is in consequence of a great extent of pneumatic receptacle that birds are enabled to bear the prodigious muscular exertion of flight. Is it not probable, too, that the repeated suspirations of the fatigued are instinctive exertions to procure a greater proportion of oxygen, by which their muscular energy

energy may be revived? I must not quit the subject of this follicle, without noticing a very curious fact well known to every one employed in the concerns of a farm-yard,—that, if the obtuse extremity of an egg be perforated with the point of the smallest needle, (a stratagem which malice not unfrequently suggests,) its generating process is arrested, and it perishes like the *subventaneous* egg. Hence sir Busick Harwood was led to suspect that the elastic fluid contained in the air-bag was oxygen, and I was induced to examine its nature. Can this curious problem be solved, by supposing that the constant ingress of fresh air is too highly exciting? A parallel example may be adduced from the vegetable kingdom in support of such an opinion. The young and tender plant, before it puts forth its roots, is often destroyed by having too free a communication with the atmosphere, by which its powers are exhausted: it is to obviate such an effect, that the horticulturist, taught only by experience, covers it with a glass, by which he limits the extent of its atmosphere, and consequently decreases its respiration, transpiration, and the inordinate actions which would prove fatal to it.

I shall close this paper with a few observations on the formation of the exterior *involucrum*, or shell, by which this microcosm is defended from external violence. We here detect a single operation, at once answering two of the wisest and most important purposes of the animal: it at once averts destruction from the individual, and contributes essentially to the preservation of its species; for, whilst it removes the calcareous matter, which, if allowed to accumulate, must render the bird incapable of flight, and defeat the best purposes of its existence, it furnishes the germ of the future animal with a strong and convenient defence. The eggs of birds are, however, sometimes destitute of this provision, which I think may arise from the secretion of calcareous matter not keeping pace with the exuberant production of the fluids of the egg. Hence we perceive this imperfection oftener occurring in strong birds, and in the months of harvest, when their food is more luxuriant and abundant. The experiments of Vauquelin, which prove that the quantity of calcareous matter voided by birds exceeds that taken in, suggested to Fordyce, that birds must require calcareous matter during their laying, and that, if the animal be deprived of it, the shell is never formed. Such a theory, however, is not only derogatory to the wisdom of nature, but illegally deduced from the experiments themselves. Are we to expect, from our imperfect

fect notions of elementary bodies, to explain the origin of every substance found in the animal œconomy, or the series of changes which it undergoes! Nature has her own laboratory, and is capable, without any foreign aid, of preparing the ingredients necessary for her productions. That a deficiency of calcareous matter in the system is the cause of the absence of the shell, no one will deny; but that this depends on some internal state, and not on the privation of lime, may be shown by the following curious circumstance. A hen, which I kept for some experiments, had its leg broken in two parts. The fracture was carefully bandaged; three days subsequent to which, several eggs destitute of shells were found on the premises. The hen had deposited no perfect eggs, nor were there any other birds from which these yolks could have proceeded: I therefore conjectured that all the *calcareous matter* designed for the formation of the shell had been employed in the regeneration of the bone. We find a similar law existing in the human species. The reunion of a bone fractured during a woman's pregnancy is often delayed until her delivery; and it is well known, that, if the horns of a deer be broken at the rutting season, it is incapable of procreating its species.

I remain, dear sir, with great esteem,

Yours faithfully,

Westminster. JOHN AYRTON PARIS.

LXV. *Some Account of the Medusæ of the Genus Equoreæ. By Messrs.* PERON *and* LESUEUR *.

Equidem et his sensum inesse arbitror, qui nec animantium, neque fruticum, sed tertiam ex utroque naturam habent, urticis (Medusis) dico et spongiis.—*Plin. Hist. Nat.* lib. ix. p. 45.

Substance.—THE substance of all the zoophytes in question presents at first sight the appearance of a kind of jelly more or less diaphanous; more or less consistent, and of various colours, according to the particular species. With the exception of the lines, lamellæ and vessels which fringe the lower surface of the umbrella, the texture of this substance seems to be homogeneous, even when observed through a strong magnifying glass: in whatever direction we tear them or cut them, the appearance does not change, nor do we discover any trace of internal vessels.

* *Annales du Muséum d'Hist. Nat.* tom. xv. p. 41.

So

So great, in short, is the density or homogeneousness of this substance, that the tenuity of the canals which pass through it, and nourish it, must be inconceivable. When exposed to the atmosphere, it resolves into a colourless liquid, very like common sea-water. We shall speak in another place of the peculiarities which this liquor presents, when we allow it to pass to the putrid decomposition, and describe the results of our analysis: it is sufficient in the mean time to observe, that this fusion of the equoreæ is so complete, that of one which weighed several kilogrammes, there scarcely remained upon the filter a few milligrammes of a membranous-like residue.

Locomotion.—Notwithstanding the singular composition of their substance, the equoreæ, like all the other species of medusæ, enjoy a power of contraction which is truly astonishing. Always active on the surface of the sea, we see them alternately locked up within themselves, and afterwards expand with more or less rapidity. We shall describe the mechanism of these motions; but let us first speak of the effect which they ought to produce with respect to the position of the animal which executes them.

In contracting themselves, the equoreæ tend to repel the column of water which is immediately in contact with the lower surface of their umbrella: by the resistance of the fluid and the decomposition of movement which it produces, the zoophyte finds itself in some measure projected in a direction contrary to that of the column of water displaced by the shock: it will therefore have changed place by a given quantity; and this quantity, every thing else being the same, will be in proportion to the force of repulsion which it will have developed. In the expansion which succeeds immediately after the contraction, the animal acts on the subjacent column of water in the same manner; and from this second percussion there results a new step for it, if we may be allowed to use the expression.

However numerous or varied may be the motions of the medusæ, we may nevertheless refer them all to these two elements, as simple as they are easy to conceive. Thus, if one of these animals wishes to rise from the bottom of the sea to the surface, it fixes itself in a vertical position, strikes from bottom to top, and rises by a series of steps, or, to speak less metaphorically, by a succession more or less rapid of contractions and dilatations, to the height which it wishes to attain. If it wishes to change the direction of its route, it inclines itself so as to make the

umbrella form with the horizon an angle more or less obtuse; and in this new situation, the direction of the shock being oblique, like that of the resistance, the animal is itself repelled, and moves in this last direction. When at the surface of the water, the vertical position can no longer have any effect but to keep the zoophyte in its place; but in order to change it, it must return to the oblique position. It is in fact in this last way that the whole of the medusæ, whose bodies are gelatinous and orbicular, swim on the surface of the water; their umbrella is never on the line of the horizon, except when they are in a state of rest.

The mechanism to which the equoreæ have recourse in order to redescend to the bottom of the sea, is still more simple than the various motions which we have described. Their substance being in fact of a greater specific gravity than that of sea-water, it is sufficient for them to contract strongly in order to sink by their own weight. In certain cases, with a view to precipitate their descent, they turn round in such a manner as to make the lower part of the umbrella uppermost; and in this position they execute the same motions as when they rise to the surface.

Methods of discovering and seizing their prey.—All these evolutions of the equoreæ have for their essential object the seizing of their food; and although less favoured in this respect than the medusæ which are provided with arms, they have nevertheless received from nature means as various and powerful for assuring themselves of the success of their effects. The filiform tentacula, of various lengths and more or less numerous, which fringe the umbrella, are endowed with the most exquisite sensibility: always in full activity around the animal, they eagerly seize the prey which they stand in need of: they wrap their tentacula around it, and carry it towards the aperture of the stomach, which dilates while the fringes or filaments around it attack the victim, and it is speedily engulfed in the fatal cavity.

To these prehensile weapons some species of equoreæ perhaps add that burning causticity which distinguishes several other medusæ, but none of those observed by us appeared to enjoy this remarkable property.

The food of the equoreæ is probably in a great measure composed of those myriads of gelatinous animalcules which float on the ocean; and the investigation of which, although but recently begun, has already unveiled so many wonders, and thrown to such a distance the boundaries of animal existence and organization; the *amphicurta equorea*,

rea, the *mesonema*, and the *phosphorifera* must at least be of this description: it seems impossible, in short, that the confined stomach of these zoophytes can receive any thing else than animalcules; the feebleness and shortness of the tentacula in the above species support this presumption: with respect to those equoreæ whose stomach is broad and deep, they have no hesitation, as we have actually observed, in devouring the largest species of beroes, of salpas, and even to the small pelagian fish which live habitually among the fuci.

Digestive system.—No organ seems less adapted to perform the most important functions of life, than the stomach of the zoophytes in question: of a soft and gelatinous substance, of great tenuity in its coats, of an extreme delicacy in its texture, it seems to be equally incapable of retaining or digesting the animals which it has received. Our uncertainty increases the further we penetrate into this singular cavity. Nowhere can the eye, even with the best optical instruments, discover any trace of those numerous suckers which we shall have occasion to describe in another place, and which fringe the bottom of the stomach of several other medusæ: all that we can see in that of the equoreæ, is that it is lubricated in all its points by a kind of gastric juice slightly viscous to the touch, and which when we apply it to the tongue immediately produces a transient sensation of pain and heat. Whatever may be the nature of this important fluid, which we shall have frequent occasion to mention in our general history of the medusæ, it, seems to be certain that it performs the chief part in the digestive system of the equoreæ: it is by means of it that the substance of the animals surprised by these zoophytes is more particularly attacked; it is this which penetrates, dissolves, and decomposes them.

Nutrition.—After having undergone this first kind of alteration, the food is probably carried into a general system of absorption and internal circulation, where, by new modifications, it ends by being assimilated with the substance of the equoreæ; but all the agents in this double office elude our research. We cannot discover absorbent pores, nor any other vessels than those which fringe the inferior surface of the umbrella, and which seem to serve the purposes of respiration, as we shall soon find. Besides, these last vessels are absolutely simple, and we do not discover any branch which is detached from them in order to penetrate the substance of the animal.

Growth and dimensions.—Whatever may be the nutritive

tive system of the equoreæ, it seems to be of considerable energy; for, independently of the rapid growth and of the large dimensions which these animals may attain, there is a peculiarity in their history which supposes a force of reparation and of assimilation very powerful.

Excretions.—If we put one of these zoophytes into a vessel filled with several litres of pure sea-water, the transparency of the liquid is soon disturbed; glarey flakes become visible in all parts of the water; they increase so rapidly, that in a very short time we see the animal expire in the midst of the excretions which he has furnished. If we take care frequently to renew the water of the vessel, the medusa will preserve all its activity; but so great at all times is the abundance of the viscous matter which transudes from all parts of its body, that the twentieth portion of water will be altered by it as speedily as the first. What can be the excretory ducts of so extraordinary a kind of transpiration? We have not been able to discover any thing satisfactory on this head; and the solution of the problem is the more difficult, because the substance of the umbrella seems to be more completely foreign to vascular organization, than such an excretion would seem to require.

Contractibility.—In treating of the locomotion of the equoreæ, we have only said a single word of the force of contraction which essentially characterizes all the animals of the great family of the medusæ: we shall on a future occasion revert to the subject of the principal seat and the agents of this valuable faculty: it is under a point of view completely novel that we are about to consider it in the following account of our investigations, and of the discovery which we think we have made in this respect.

Respiration.—The contractibility in question is manifested by a phænomenon so striking, that it is not astonishing that most writers have made particular mention of it. All are agreed in assigning a peculiar system of locomotion and progression to the alternate contractions and dilatations of these zoophytes. This assertion is doubtless correct, and the details which we have ourselves given on this subject cannot leave any reasonable doubt as to this essential point of the history of the medusæ. But are these motions, so constant and so regular, exclusively dedicated to this last function? This is the problem which we shall try to resolve.

If we observe any medusa on the surface of the sea, and under any given circumstances, we see it alternately

contract

contract and dilate its umbrella. If, we study the relation between these oscillations and the progression of the zoophyte, we shall soon find that in some cases, and even when they are most lively, these oscillations are not followed by any removal of the animal who makes them: if we remove the medusa from the sea, and place it in a glass vessel sufficiently provided with fresh sea-water, and the diameter of which is such in proportion to the size of the umbrella that progression is physically impossible, the oscillations will nevertheless take place with the same appearances as when the medusa swam at large on the waves. If the animal descends to the bottom of the vessel, and attaches itself to it, still in this last case the same motions will be continued; and although less energetic in appearance, they will always present the characters of this regular succession which we have indicated. If we remove the zoophyte from its natural element, and place it on the hand, on a table, on a stem, or any other solid body, it will still continue to move: the oscillations will doubtless appear more feeble, because the organs which cause them will be as it were pressed under the weight of the body, but they will not be less constant or less regular: if, with certain precautions to be afterwards mentioned, we cut into several pieces the umbrella of a medusa, each of the slices will continue to move for some time.

If, after having ascertained the existence of these oscillations under all the relations which we have indicated, the observer endeavours to ascertain how far their motions are regular, he will soon be convinced that the contractions and dilatations are isochronous, *i. e.* that equal intervals of time correspond with equal numbers of each of them; that, in similar circumstances, the quantity of these oscillations is the same for individuals of analogous proportions; that it is greater, every thing being similar in other respects, as the animals of each species are smaller and probably younger; that these pulsations become less frequent and feebler in proportion as the vital energy of the animal diminishes; but that, in this last case also, they preserve their isochronous motion, that they continue even some time after the general death of the individual, and that they may be kept up or excited by various physical and chemical agents.

Spallanzani had already made the greater part of these experiments; and although at the time we were strangers to the labours of this great man, we were led to the same results,

results, by the observation of several thousands of medusæ of various kinds collected in different seas.

Now we may venture to ask physiologists, how they can conceive that so many eminent characters, that so much order and regularity, can belong exclusively to this same system of locomotion, which in all the other families of animals seems to require from nature, and to have actually received from her, the greatest mobility in its principle, the greatest anomaly in its developments, the greatest independence and versatility in its immediate agents? How can we in fairness refuse to acknowledge the numerous characters which we have indicated, a true system of general contractibility, the locomotion of which is in truth one of the most sensible results, but which seems to belong in a still more important manner to the very essence of the life of the medusæ?

If, in short, we glance at the numerous series of beings which compose the animal kingdom, we soon find that, whatever may be the differences of forms and of organization which they affect, all of them have nevertheless a certain number of common functions, without the union of which their existence would be as it were impossible to conceive. In the more perfect animals, each of these grand functions has its peculiar seat, its distinct organs, and particular laws; but this could not be the case with those anomalous species on which nature seems in a great measure to have attempted some huge animal creations: the singular substance of these less perfect species, the homogeneousness of their texture, the simplicity of their organization, reduced to the first element of life, every thing in them opposes the distinction, and above all the multiplicity of the organs. Subordinate from that moment to common agents, the functions most essential to existence may be easily mistaken; because they are confounded in their effects, as in the principle which determines them, and which supports them.

This principle seems to be, in the medusæ, the very contractibility in question. Who is there, in fact, who cannot see how favourable these motions, so continuous and so regular, of systole and diastole, are to the circulation of liquids in the most delicate vessels of the umbrella? How greatly do they aid digestion and nutrition! How great is their influence over the abundant secretions just mentioned! How great, in short, is their analogy with these movements of inspiration and expiration, which are exhibited

bited to us by most of the other productions of the animal kingdom!

In both cases, the progress of the phænomena is similar, their modifications are comparable; and their results are equally useful and equally indispensable to the preservation of the beings which produce them. In this way, by the play of their lungs and gills, the mammiferæ and fishes can renew the portion of air or liquid which surrounds or penetrates them: in the same way also, by alternate contractions and dilatations of their umbrella, the medusæ have the faculty of replacing by means of new water that which is immediately in contact with them; and this change is perhaps still more indispensable to them than to the animals with which we compare them in this respect. The abundance of their excretions is in effect so considerable, nature is so particular in this respect, that they could not live long, and they would actually die in the purest sea-water, if we neglected to change it very often.

We think there ought to be no hesitation, therefore, after the important facts which we have established, in granting to the oscillations of the medusæ, independently of the locomotive power, which is their peculiarity, two other analogous functions; the one being the system of general contractibility, the other that of the respiration of the most perfect animals.

This interesting analogy does not seem to have escaped the ancients: at least we may conjecture thus much from the Greek names of *Hali Pleumon*, *Pleumon Alios*, *Pneumon Thalassios*, *Pneumon Thalastios*, *Pneumon Thalattios*, by which Aristotle, Dioscorides, Kiranides-Kirani, and some others have designated the medusæ. The Romans also gave the denomination of *Pulmo marinus* to these same zoophytes, a name which has since been used by modern authors; such as Gyllius, Massarius, Ruellius, Cordus, Rivius, Belen, Matthiolus, Aldrovandus, and Merret. The Italian naturalists call the medusa *Polmone marino*, and the English, Dutch, and Germans, know it by the name of *Sea-lungs*. The *Poumon marin* of the French authors refers to the same idea; which rests on facts so simple, and on a comparison so natural, that it can scarcely be conceived that, among the many eminent naturalists who have recently written on the medusæ, none have directed their attention to this curious part of their history. Spallanzani himself does not appear to have even suspected the interesting connexion in question.

Every

Every thing that has been now advanced as to the respiration of the medusæ, supposes that there does not exist in them any kind of apparent respiratory organ, and this in fact is the case with the greatest part of these singular animals: there are several, however, which form an important exception to this rule, and which are evidently provided with gills more or less perfect. The equoreæ in this respect present a set of very curious observations. In fact, in those which constitute our first sub-genus, we see at first a singular circle of simple lines, which we have described in another place, and which are to be met with in a very small number of other medusæ; soon these lines extend, develop, and are transformed into follicules, and into folds so numerous, so delicate and flexible, that we cannot longer doubt that they perform an important part in the history of those animals which have received them from nature; and when, upon a closer inspection of these organs, we discover, as Forskahl has already done, that these follicules are susceptible on being brought together in pairs of forming a multitude of canals, through which water may circulate from the edge of the umbrella to the base of the stomach, it is very sceptical not to regard these innumerable follicules as so many true bronchiæ, perfectly analogous in their structure, distribution, and use, to those of several other marine animals.

Thus, it is not only upon a numerous and incontestable series of analogies that the fact of the respiration of the medusæ is founded; the very existence of the organs appropriated to this function cannot leave any reasonable doubt as to the important discovery which we have made. We may even add, in order to remove all kind of uncertainty as to this grand fact in natural history, that there are species of medusæ of an organization still more complex than those above described, and in which we may easily follow all the details of the respiratory system. Such are among others the *rhizostomes*, the *aurelliæ*, the *cyaneæ*, the *chrysaoræ*, and most of the other polystome medusæ. When we come to speak of these last, we shall exhibit in a series of engravings every part of the mechanism of this important function, of which the equoreæ furnish but an imperfect illustration.

[To be continued.]

LXVI. *Re-*

LXVI. *Report from the* Vaccine Establishment, 1811.

To the Right Hon. Richard Ryder, *principal Secretary of State, Home Department, &c. &c. &c.*

National Vaccine Establishment, March 7, 1811.

Sir, The Board of the National Vaccine Establishment have the honour of submitting to your consideration a statement of their proceedings during the year 1810.

They have to report to you, that the surgeons of the nine stations established in London have vaccinated during the last year 3,108 persons, and that 23,362 charges of vaccine lymph have been distributed to various applicants from all parts of the kingdom; being an excess of nearly one-third in the number of persons vaccinated, and in the number of charges of lymph distributed, above that of the preceding year.

They have further to report, that no case of failure has occurred, in any individual vaccinated by the surgeons of the nine stations, since the commencement of this establishment; that the few instances of failure, submitted from other quarters to the investigation of this board in the last year, have been asserted without sufficient proof; that such reports of failure as have been received from the country, have been ascertained to rest upon imperfect evidence.

They have great satisfaction in being able to state the favourable result of vaccination in the Royal Military Asylum for the Children of Soldiers; and in the Foundling Hospital. At the establishment of the *former* of these charities, in the year 1803, vaccination was introduced, by order of government; and it continues to be practised at the present time. During the whole of this period, this institution, which contains more than eleven hundred children, has lost but *one* of them by small-pox, and that individual had not been vaccinated, in consequence of having been declared by the mother to have passed through the small-pox in infancy. In the *latter* institution, *no* death has occurred by small pox, since the introduction of vaccination in the year 1801; from which period, every child has been vaccinated on its admission to the charity; and in no instance has the preventive power of vaccination been discredited, although many of the children have been repeatedly inoculated with the matter of small-pox, and been submitted to the influence of its contagion.

They have also the satisfaction of being able to state, that similar success has attended the practice of vaccination at the Lying-in-Charity at Manchester, where, in the space

space of nine years, more than nine thousand persons have been effectually vaccinated; and that by a report received from Glasgow, it appears, that of fifteen thousand five hundred persons who have undergone vaccine inoculation in that city during the last ten years, no individual has been known to have been subsequently affected with small-pox.

It is with a very different feeling that the board are induced to call your attention to the number of deaths from small-pox, announced in the bills of mortality of the year 1810, amounting to 1,198, which, although great, is considerably less than it had been previously to the adoption of that practice.

The board are persuaded that this mortality has arisen from contagion having been propagated by inoculated persons, of the poorer class, whose prejudices against vaccination are kept alive by false and mischievous hand-bills, denouncing various imaginary and feigned diseases against all those who have undergone vaccination: and the board have reason to believe, that these bills are issued by persons, in several parts of London, who derive emolument from small-pox inoculation.

The board have been induced, by these considerations, to address the information contained in the preceding paragraphs to the committees of charity-schools; and to submit to them the propriety of introducing vaccination into their respective establishments, and among the poor in general.

Besides the duty of superintending the practice of vaccination in London, they have been engaged in an extensive correspondence with several vaccine establishments in the provincial towns; and they acknowledge with pleasure, the readiness with which many of these bodies have communicated information.

From these sources, They are enabled to state, that the practitioners of the highest respectability in the country have been earnestly engaged in promoting the practice of vaccination, by the weight of their authority and example: That in the principal county towns, gratuitous vaccination of the poor is practised, either at public institutions, or by private practitioners, on an extensive scale: That, among the superior classes of society in the country, vaccination is very generally adopted: That the prejudices of the lower orders, excited against the practice by interested persons, still exist, but appear to be gradually yielding to a conviction of its benefit.

The

The information received from Scotland is of a very favourable nature, and it will appear from the annexed Reports of the College of Physicians, the College of Surgeons of Edinburgh, and of the Faculty of Physicians and Surgeons of Glasgow, that the practice of vaccination is universal among the higher orders of society; and that, in the opinion of these learned bodies, the mortality from small-pox has decreased in proportion as vaccination has advanced in that part of the united kingdom.

The Reports of the Vaccine Establishment instituted at Dublin, under the patronage of the Lord Lieutenant, state, that vaccination continues to make progress in that city, and in Ireland generally; and that the prejudices against it are subsiding.

The board have also received very favourable accounts of the progress of vaccination in India; and they have the honour to subjoin a Statement, from which it appears, that by vaccination the ravage of small-pox has been repeatedly prevented, and the disorder exterminated in the Island of Ceylon.

The board, guided by the inferences which facts reported to them from undoubted authority and actual observations have furnished, declare their unabated confidence in the preventive power of vaccination, and their satisfaction with the gradual and temperate progress by which this practice is advancing; that the local and constitutional maladies, which frequently follow the small-pox, rarely (if ever) succeed to vaccine inoculation; that it produces neither peculiar eruptions nor new disorders of any kind; and that they are of opinion, that by perseverance in the present measures, vaccination will in a few years become generally adopted.

The board have great pleasure in stating that the money granted by parliament during the last session has been sufficient to defray the expenses of the year 1810; and they are of opinion that the same sum will be adequate to the expenditure of the current year.

By order of the Board, L. Pepys,
James Hervey, Register. President.

Appendix *to the* Report *from the National* Vaccine *Establishment*;—viz.

The *Report* of The College of Physicians of *Edinburgh*.
The *Report* of The College and Corporation of Surgeons, *Edinburgh*.

The *Report* of The Faculty of Physicians and Surgeons of *Glasgow:* with
The *Report* also from The Island of *Ceylon.*

Excerpt *from the* Minutes *of the Meeting of the Royal College of* Physicians *of* Edinburgh, *held 22d Feb.* 1811.

A letter from the Board of the National Vaccine Establishment, to the Royal College of Physicians of Edinburgh, containing queries respecting the *effects which the practice of inoculation has produced in Scotland*, and requesting information particularly upon certain points, having been considered, the Royal College of Physicians are of opinion,

First, That the practice of vaccination has rather decreased of late among the lower orders, owing to misrepresentations, prejudices, and ignorance; but that it continues to be very universally adopted by the other classes of the community in Scotland.

Secondly, That no bad effects can be ascribed to vaccination.

Thirdly, That the practice of vaccination is very universally approved of by the profession in Scotland.

Fourthly, That the small-pox inoculation has been rarely practised of late in Scotland, and only with the view of ascertaining the security which previous vaccination may have afforded.

Lastly, That few cases of natural small-pox have occurred within their practice, and these among the lower orders only; and they are persuaded that the mortality arising from small-pox has very greatly decreased.

Extracted by
Alex. Boswell, Clerk.

Report *of the College and Corporation of* Surgeons, Edinburgh.

Sir,—I am desired by the Royal College and Corporation of Surgeons of Edinburgh to acknowledge the receipt of your letter of the 7th instant, and to forward the following answers to the queries of the Board of the National Vaccine Establishment.

Query 1st.—Does the practice of vaccination increase?

Answer.—The practice of vaccination is universal among the higher orders of society throughout Scotland, and among the lower orders it daily increases.

Query

Query 2d.—Is it the opinion of the College, that any bad effects can justly be attributed to vaccination?

Answer.—It is the opinion of the College, that no bad effects whatever can justly be attributed to vaccination.

Query 3d.—Is this practice approved of generally by the medical profession and by the public?

Answer.—The practice of vaccination is generally approved of by the medical profession and by the public throughout Scotland. There is, however, observed among the lower orders of the community a degree of apathy, which, although they certainly approve of the practice, prevents them from bringing forward their children for vaccination, unless urged to do so by the appearance of small-pox in their neighbourhood.

Query 4th.—To what degree is small-pox inoculation practised?

Answer.—The College know of very few instances in which small-pox inoculation has been practised or even required to be practised by any of its members during the last six or eight years, unless as a test of perfect vaccination. And they believe that the same is generally the case with the medical practitioners throughout Scotland, except in a few situations in which small-pox have appeared, and where at the time no vaccine matter could be obtained.

Query *last*.—Has the mortality from small-pox been observed to decrease throughout Scotland, since vaccination was introduced?

Answer.—As bills of mortality are not general in Scotland, it is impossible to ascertain the proportion of deaths from small-pox; but from the best information the College have been able to obtain, they believe that the mortality from small-pox has greatly decreased since the introduction of vaccination. I have the honour to be, sir,

Your most obedient and most humble servant,

A. Gillespie,

President of the Royal College and Corporation of Surgeons of Edinburgh.

Edinburgh, Dec. 26, 1810.

To James Moore, Esq.

REPORT *of the Faculty of* Physicians *and* Surgeons *of* Glasgow.

SIR,—THE faculty of Physicians and Surgeons in Glasgow, in answer to your queries, beg leave to observe,

1st.—That in this city the practice of vaccination has

increased much; for in the greatest number of families, soon after the birth of a child, it is now almost uniformly vaccinated. To this practice, it is believed, there are few exceptions, except perhaps in the lowest rank.

2d.—The faculty, from their experience, are of opinion, that no bad effects can be justly attributed to vaccination.

3d.—The practice of vaccination is generally approved of here by the medical profession, and also by the public.

4th.—Small-pox inoculation is almost never practised here, except in a few rare instances after vaccination, to satisfy those who entertain any apprehension of the person vaccinated being still liable to the small-pox.

Lastly.—The mortality from small-pox has greatly decreased in this place since the introduction of vaccination, and also throughout Scotland.

N. B. Since the middle of May 1801, till this date, the Faculty of Physicians and Surgeons have been in the practice of gratuitously vaccinating in their hall, once a week, all who come there with that view; and by a register which is kept, find the number vaccinated in that way, during the above period, amounts to 14,500; and, as far as is known, vaccination in all these has succeeded.

I have the honour to be, sir,
Your most obedient servant,
Robert Freer,
Preses of the Faculty of Physicians and Surgeons.

Faculty Hall, Glasgow,
Jan. 11, 1811.

James Moore, Esq.
Director of the National Vaccine Establishment, London.

Report *from the Island of* Ceylon.

TO THE EDITOR OF THE CEYLON GOVERNMENT GAZETTE.

Sir,—I beg leave to subjoin, for more general information, an abstract of the number of patients vaccinated in the different districts on Ceylon, during 1809, amounting to 25,697; which added to 103,035, the number vaccinated in former years, makes a total of 128,732 persons who have been officially reported to me by the respective superintendants and vaccinators, as having regularly passed through the vaccine disease since its first introduction into this island in 1802: besides a few others inoculated by individuals not belonging to the Vaccination Establishment.

Agreeable to the best information I have been able to obtain, the small-pox has not existed in any part of this island since February 1808, till October last, when the dis-

ease

ease was brought to Jaffnapatnam by a country boat from Quilon, on the Malabar coast. The contagion spread to a few individuals who had not been vaccinated in the Pettah of Jaffnapatnam, and by means of a civil prisoner, was introduced into the jail at that place; but its progress there was immediately arrested by the removal of the infected persons, and the indiscriminate vaccination of all the other prisoners.

By a late report from Mr. Stutzer, superintendant of vaccination at Jaffnapatnam, it appears there were only six individuals ill of the small-pox in that district; and it has found its way to no other part of the island, except Putlam, where a coolie from Jaffna was taken ill with small-pox in December last, but has since recovered, without communicating the disorder to any other person.

The vaccine disease has now been so extensively diffused throughout this island, that while the inoculations continue so numerous as at present, we can have no reason to apprehend that the contagion of small-pox will ever spread epidemically in any part of the British possessions on Ceylon; and its occasional appearance here has the good effect of proving the preservative efficacy of the vaccine, and of rousing the natives from their apathy on the subject, as exemplified at Jaffnapatnam, where 1,830 people have been inoculated during the last two months, and among them several Bramins, men and women, who had hitherto declined submitting to the operation.

I shall only add, that with a view of proving the permanency of the preservative efficacy of cow-pox, and the continuance of the purity of the virus on this island, Mr. Stutzer has, at my request, in November and December last, inoculated with small-pox matter several patients who had passed through the vaccine disease in 1804 and in 1809, all of whom have resisted the contagion.

I have the honour to be, sir,

Your obedient servant,

Colombo, 24th Jan. 1810.

Thomas Christie,

Medical Sup. General.

Abstract of the Number of Patients Vaccinated in the different Districts on Ceylon, during the Year 1809.

Superintendants.	Districts.	Vaccinators.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
A. High, esq.	Caltura	J. W. Pieterse	141	92	129	146	129	127	143	137	148	140	118	114	1,564
	Colombo	H. W. Schemmelkettle	103	234	237	122	247	107	150	123	152	158	169	134	1,936
	Negombo	M. Mark	153	108	126	118	102	105	46	88	87	98	95	91	1,217
	Chilau	J. H. Vansanden	31	29	38	29	20	26	43	74	56	55	67	22	490
	Calpentyn	B. H. Toussaint	43	50	35	20	—	—	22	56	—	34	44	28	332
	Putlam	J. L. Janzen	61	59	62	57	70	69	41	40	33	39	28	34	593
J. A. Stutzer, esq.	Mannar	H. Mattheist	104	103	117	121	151	156	157	90	97	117	89	143	1,445
	Jaffna	C. Keegell, 1st	218	248	229	209	182	313	334	264	240	263	429	537	3,466
	Jaffna	N. Claass, 2d	255	223	324	343	271	323	291	293	298	291	335	569	3,816
	Mullativo	C. Schneider	75	61	58	64	69	66	37	39	40	24	22	20	575
J. Bate, esq.	Trincomallie	F. Vansanden	107	58	42	106	120	69	115	88	64	61	59	66	955
	Batticalva	J. W Seyp	85	129	195	180	122	99	104	103	108	104	78	64	1,371
M. Reynolds, esq.	Hangbantotte	C. G. Hopman	13	14	17	10	12	13	13	13	14	19	16	14	168
	Tangalle	C. Gersse	121	108	177	116	133	140	182	242	215	267	132	195	2,028
	Matura	F. W. De Hoedt A^s	415	417	433	445	405	372	382	394	422	322	307	523	4,837
	Point de Galle	J. Zebyrands	117	71	33	61	78	64	70	80	87	93	79	71	904
		Total	2,042	2,004	2,252	2,147	2,111	2,049	2,130	2,124	2,061	2,085	2,067	2,625	25,697

Colombo,
24th January 1810.

Thomas Christie,
Med. Sup. Gen.

LXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 2.—The conclusion of Mr. Bell's paper on the measurement of the bones of the head, particularly the skull, was read. It contained some observations on the relative dimensions of the different parts of the head, with reference to the mental powers of the individual, all of which tended to prove that some positive knowledge might be attained on this subject, and that the figure of certain exterior organs does undoubtedly indicate some quality of mind or mental affections. Mr. Bell compared the facility and advantages of his mode of measuring the skull over that adopted by Camper, Blumenbach, and others, as well as its superior accuracy.

On the 9th a paper by Mr. Brande was read, detailing a series of experiments on a vegetable wax sent from the Brazils to Lord Grenville, and by his lordship handed to Sir Joseph Banks to be chemically investigated. Mr. B. found this vegetable substance to be as perfect wax and fit for every purpose as that of bees, except that he did not succeed in his efforts to bleach it. The vegetable wax found by Humboldt in Peru was the product of a large tree growing on very elevated situations; but it contained a considerable portion of resin. The present vegetable wax is found on middle-sized trees in low situations in Brazil, and is perferfectly free from resin; it is as combustible and fusible as bees-wax, and may be used for the same purposes. The natural history of the tree, and the quantity of wax which it yields, are yet unknown; but it is designed to investigate these further, when it is found to be applicable to domestic use. At present it is supposed to exist in such quantities as may render it an important article of commerce.

On the 16th and 23d, part of a long paper by Mr. Travers, demonstrator of anatomy in Guy's Hospital, was read, on wounds in the intestines. The author related a series of experiments on dogs, the intestines of which were wounded in various manners, some of which recovered, and were afterwards killed to ascertain the way in which the punctures or wounds had adhered; others died of the wounds, and were then dissected. In general, it appears that in small wounds of the intestines there is no extravasation, and that the incisions heal very rapidly by adhesion. Much of this, however, depends on the circumstance whether the wound be

 longi-

longitudinal or horizontal, so that that the peristaltic motion may not obstruct the natural progress of adhesion. If the mucous membrane likewise be punctured, inflammation and not adhesion takes place.

ROYAL INSTITUTION.

Mr. Davy's Lectures on Geology.—No. I.

Mr. Davy, after some introductory observations, pointed out two distinct arrangements of rocks—one, characterized by a crystalline texture, by a stratification approaching to the perpendicular in its direction, and by a total want of organic remains; the other, known by the horizontal position of its strata, and by the intermixture of petrifactions and water-worn stones. The first arrangement constitutes the primary class of rocks, and the last the secondary. Both are traversed by veins, which were formerly empty fissures, but are now filled up, and become the repositories of metallic ores. As the same rocks, in all parts of the globe, are similarly associated, and contain similar metallic deposits, their relations and transitions form the most important part of geology.

Mr. Davy showed the excellence of the present order of things, and that the irregularities of the surface of the earth were wise contrivances. He pointed out the changes to which rocks are at present liable from the action of the air, sun, and the vicissitudes of the seasons, and noticed the operations counteracting this destructive process, such as the formation of islands at the mouths of rivers, vast productions of coral, and islands the result of submarine fires; and he showed that the degradation of the solid rock itself had beneficial consequences—that it gave rise to new soils, to the fertilization of barren tracts, to the filling up of lakes, &c.

Mr. Davy deferred the examination of the different hypotheses advanced respecting the past alterations of the globe to the concluding part of his course. The two principal hypotheses are the Plutonian and Neptunian. Hooke started the first, in which our continents are supposed to be in a continual state of decay and of renovation, the agencies of the elements being the destructive powers, and the action of a great central fire on the detrition of our land accumulated in the bed of the ocean, the renovating power. The central fire, its principal engine, has been the object of great objection.

Mr. Davy remarked, that the source of this imaginary fire

fire might be attributed to the existence of the earths in their metallic state in the interior, acted on by air and water, and thus supplying fuel, and that the reproduction of these metals might be owing to internal electrical currents. In the Neptunian hypothesis, water is the general solvent, and supplies the place of fire in the Plutonian; and our continents are supposed to be derived from a fluid chaos, the primary rocks by crystallization and deposition, and the secondary by a simple deposition at a later period, after the sea was stocked with inhabitants. Beside these two, many others have been resorted to. Leibnitz and Whiston, for instance, imagined a comet to have been concerned in producing the present appearance of things, by elevating the ocean, inundating the continents, and by heating its waters giving them new solvent powers.

Mr. Davy pointed out two grand circumstances connected with this inquiry; 1st, Alterations produced in secondary rocks by causes acting from above, such as the opening of valleys, the sweeping away of strata, &c. without the parallelism of the remaining strata being altered.—2dly, The derangement of the primary rocks by causes apparently acting from below. He asserted that more than one system of causes was necessary to account for all the phænomena, and that the practice of assigning them all to one was faulty: he advanced several illustrative instances in which unity of effect is the result of a variety of causes.

Mr. Davy recommended to those who wished to become acquainted with geology, the examination of geological collections, and the perusal of geological writings, particularly of those enlightened observers, De Saussure, Dolomieu, Humboldt, and Jameson.

He stated that the science, independent of the healthy employment it gives to the mind, is of great importance in a practical point of view; that it very nearly concerns the miner, engineer, and drainer, and even the farmer and architect—that it discloses a variety of indications highly useful in their respective pursuits:—to the miner, the rocks containing metallic veins and coals; to the engineer, the association of hard rocks with soft; to the drainer, the intersection of a country by hard dykes, or veins impermeable to water; to the farmer, the best places for finding limestone, marl, and clay; and to the architect, the most durable stones for buildings: and he mentioned several instances of the serious evils arising from a want of geological knowledge.

"The person who is attached to geological inquiries," says Mr. Davy,

Mr. Davy, "can scarcely ever want objects of employment and of interest.

"The ground on which he treads, the country which surrounds him, and even the rocks and stones removed from their natural position by art, are all capable of affording some degree of amusement—and every new mine or quarry that is opened, every new surface of the earth that is laid bare, and every new country that is discovered, offers to him novel sources of information.

"In travelling, he is interested in a pursuit which must constantly preserve the mind awake to the scenes presented to it—and the beauty, the majesty, and the sublimity of the great forms of nature, must necessarily be enhanced by the contemplation of their order, their mutual dependence, their connexion as a whole.

"The imagery of a mountain country, which is the very theatre of the science, is in almost all cases highly impressive and delightful; but a new and a nobler species of enjoyment arises in the mind, when the arrangement in it, its uses, and its subserviency to life are considered.

"To the geological inquirer, every mountain chain offers decided proofs of the great alterations that the globe has undergone."

Lecture II.—In this lecture Mr. Davy described two species of characters to be attended to in the study of geology—one, those which insulated specimens of rocks present, such as physical qualities, constituent parts, &c. the other, the aspect of rocks considered as great masses, or their general features, such as outline from colour, stratification, &c.

In the primary order of rocks, he pointed out and described six classes. These, he observed, included all rocks strictly belonging to this order. He excluded argillaceous and siliceous schist, because water-worn stones or shells occur in them, and the topaz rock of Werner, as having more the appearance of part of a vein. Granite, micaceous schist, sienite, serpentine, porphyry, and marble, are primary rocks.—All these arrangements are constituted by a few crystalline substances, which are principally quartz, feldspar, mica, hornblend, talc, and calcareous spar. Thus granite is an aggregate of crystal, of quartz, mica, and feldspar, and sienite of quartz, feldspar, and hornblend—micaceous schist, of quartz and mica—serpentine, of skiller spar, talc, and feldspar, containing veins of steatite.

The mechanical constituents of rocks are few; but their chemical elements are still less numerous. When subjected to analytical processes, they afford silex, alumina, magnesia,

magnesia, lime, fixed alkali, and oxide of iron. These substances, variously combined, give rise to the great variety of the forms and appearances of their crystals; and Mr. Davy's late discoveries prove that the earths and alkalies have metallic bases, and that they are compounds of these bases and oxygen.

In the arrangement of the primary rocks a certain order and relationship is generally to be observed—granite is the highest and deepest rock, it forms the summit of the loftiest mountains, and seems to be the foundation of our continents and islands, and is usually covered by geogneis (itself a species of granite), micaceous schist, or sienite. Serpentine and marble occupy the middle stations of mountain chains, and are oftener found upon micaceous schist than upon granite.

Porphyry is mostly associated with granite, and is frequently immediately incumbent upon it. The primary rocks constitute the principal solid part of the surface of our globe, and form the loftiest mountains, and their geographical position is admirably adapted to preserve the order and œconomy of the system.—The heights of mountains in general diminish from the equator towards the poles. In hot climates the effects of mountains are to lower the temperature of the subjacent countries, and thus most of the tropical regions are rendered habitable. Where mountains do not occur, as in Africa, there are sandy deserts;—mountains too are the sources of streams and rivers, they attract clouds, condense vapour into water, and pour it into the valleys and plains; they modify the course of the winds, and shelter the lowlands.

In all systems for explaining the formation of primary rocks, a fluid state either from igneous or aqueous fusion is assumed, but it is not yet explained. Mr. Davy remarked, why different crystals should separate in the same mass. The crystals produced either from solutions or by slow cooling in artificial or natural operations are uniform, and not of different species, like those of the primary rocks; so that the solution of this grand problem, if it be capable of solution, must be gained from the improvement of chemical science. Nature may produce effects by powers which have not as yet been discovered, and her resources should never be estimated by our operations.

Mr. Davy, in the course of this lecture, mentioned the applications of the substances derived from the different primary rocks, to the uses of common life, and particularly pointed out their applications to the purposes of architecture.

ture. Granites, porphyries, and sienites, are the most durable stones; micaceous schist and serpentine are much more liable to decay. The most perfect of the works of the Egyptians, those least injured by time, are of granite. This stone or porphyry should be used for all great public monuments. Mr. Davy, in touching on this subject, said, he could not avoid introducing a few observations on the little attention paid to such public memorials in this great country. Yet, said he, our materials are copious; our harvests of glory are as rich, nay even more abundant than those of the great elder nations. Why should the spirit be wanting by which they are to be gathered in and made permanent? We have had philosophers who are the glory of the whole human race; heroes and statesmen, rivals of the illustrious of Athens and of Rome. Yet this metropolis offers no great durable tribute of respect to our science, and our naval and military glory; and in a thousand years, though there may be a new and more magnificent city on the banks of the Thames, yet there will scarcely be a wall of what we now behold standing; nothing to speak to posterity of what we are in these memorable times; in our philosophy, the guides; in our literature, the instructors; and in our politics, the saviours of Europe.

Nor would such works be devoid of immediate utility and beneficial effect.

A few columns raised to the illustrious dead; a few national laboratories, or museums, devoted to the memory of great men, and to the use of students, would rise as landmarks of fame, would continually excite to excellence. No motive for exertion is so strong as that founded upon the sympathy of the good and wise; no reward so sweet as that of being held up to public admiration, as a benefactor of the species; no glory so pure, so calculated to awaken great minds, as that of immortality!

Lecture III.—In this lecture Mr. Davy described the secondary rocks, or the rocks containing fragments of the primary substances, and the remains of organized beings. He divided these rocks into three families, and illustrated their nature and their arrangements by specimens and by paintings. The first family, he stated, contains nearly the same elements as the primary rocks. It comprehends the secondary granites, micaceous schists, sienites, porphyries, &c. They differ from the primary rocks of the same name, by the occurrence of fragments, principally of water-worn quartz; and they do not appear in considerable masses, but in

in beds or veins: these rocks are much more abundant in metallic ores than the primary rocks; they are not so proper for architectural purposes, being liable to decomposition; but when decomposed they afford clays and earths, which form the basis of the finest porcelain, and which are applicable to other purposes in the arts. In this family no organic remains have as yet been found.

The second family of the secondary rocks contains the impressions of marine exuviæ, less crystalline matter than the first family, and fragments in greater abundance: it is composed of the argillaceous and siliceous schists, the schistose porphyries, trapps, grawackes and breccias. The mechanical elements of all these rocks, independent of the fragments they contain, are principally feldspar, quartz, hornblende and chlorite; feldspar serving as a cement to connect the others together. The rocks of this family are variously associated with each other, and sometimes with limestone; the position of their strata is irregular, it is seldom horizontal or vertical, but generally more or less inclined. Coal, without bitumen, is peculiar to this family.

The third family, besides marine exuviæ, contains the impressions of fishes and vegetables, and the remains of quadrupeds; it is also distinguished by a horizontal stratification, and by the uncrystalline appearance of most of its rocks, excepting basalts; shell limestones, sand stones, shales, bituminous coal, and basalts belong to this family. Basalts are aggregates of small crystals of hornblende and feldspar.

Mr. Davy was of opinion that much remained to be learnt from chemistry respecting the formation of this family of rocks: he showed that all the phænomena they present are not explicable on the supposition of the action of one simple cause; that fire or water alone is insufficient, but that their joint agencies are more agreeable to the visible effects.—He introduced some observations on the tendency of geological speculations, when attempts are made to connect them with the truths of revelation. He quoted Lord Bacon on the occasion, and urged the separation of divine things from human. The ordinances of the Creator, he said, were not to be judged of by the fleeting opinions of man; their theories and their hypotheses are in continued fluctuation, and should therefore never be blended with that which is divine and immutable.—The secondary rocks form the beds on which our fertile and cultivated soils are situated. They are abundant in fossil coal and in mineral veins, and they afford a number of substances con-

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nected with our wants and our comforts. They all exhibit proofs of the agency of powerful causes, which destroyed and dissolved a considerable part of the ancient world; and which by successive operations effected a *less perfect* consolidation of its materials, in some cases almost assimilating them to their original appearances, but always impressing on them the stamp of change, always constituting them monuments of the great destruction and renovation that took place in the obscure and early epochs of the history of nature.

[To be continued.]

LXVIII. *Intelligence and Miscellaneous Articles.*

The Royal Medical Society of Edinburgh will give a gold medal of five guineas value, to the author of the best experimental essay in answer to the following question:

"Does any decomposition of acids and alkalis take place on their uniting to form neutral salts, according to an opinion lately advanced by Mr. Davy, in respect to the muriates?"

Honorary, extraordinary and ordinary members are alone invited as candidates. The dissertations are to be written in English, Latin, or French, and to be delivered to the Secretary on or before the first day of December 1812. The adjudication of the prize will take place in the last week of the following February.

To each dissertation shall be affixed a motto, which is also to be written on the outside of a sealed packet containing the author's name and address.—No dissertation with the author's name will be received; and all dissertations, except the successful one, will be returned with the sealed packet unopened.

Mr. James P. Tupper, member of the Royal College of Surgeons and Fellow of the Linnean Society, has in the press a work to be entitled "An Essay on the Probability of Sensation in Vegetables; with Additional Observations on Instinct, Sensation, and Irritability."

M. Carnot, the mathematician, who during the French revolution was for a considerable time minister at war, lately composed, by the direction of the French emperor, a work for the instruction of the pupils of the corps of engineers "On the Defence of Fortified Places." A second edition of this treatise has recently appeared on the Continent.

nent. It has for its motto, "In the defence of fortified places, bravery and prudence when not united will not avail; but, when united, will effect every thing." In this work M. Carnot gives an account of all the most famous sieges, from those of Tyre by Alexander the Great, to those of modern times.

LECTURES.

Dr. Adams will commence a Summer Course of Lectures on the Institutes and Practice of Medicine, about the beginning of June. Particulars may be known at his house, No. 17, Hatton-Garden.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, June 3, at Ten o'clock in the morning, at his house in New Bridge-street, where further particulars may be had.

On Monday morning, June 3, at No. 9, in George-street, Hanover-square, a Course of Lectures on Physic and Chemistry will recommence, viz. the Medical Lecture at Eight, and the Chemical at Nine o'clock, by George Pearson, M.D., F.R.S., Senior Physician to St. George's Hospital, of the College of Physicians, &c. &c. Proposals may be had at No. 9, George-street, and at St. George's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

To Wm. Everhard Baron Van Doozrick, of Broad-street, Golden-square, in the county of Middlesex, for an improvement in the manufacture of soap, to wash with sea-water, with hard-water, and with soft-water.—April 27, 1811.

To William Caslon, the younger, of Salisbury-square, in the city of London, letter-founder, for an improvement in the register belonging to a mould for casting types.—April 27.

To George Alexander Thompson, gent. of 36 Parliament-street, for his machinery for the purpose of dragging, locking, and scoiting the wheels of carriages.—May 1.

To Stedman Adams, of Connecticut, one of the United States, (now residing in London,) esq. for his method for the application of mechanical powers to the propelling ships and vessels of every description through the water.—May 1.

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For May 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	54	62°	50°	29·55	65	Fair
28	54	60	50	·50	49	Fair
29	50	54	46	·40	24	Showery
30	48	58	50	·75	42	Cloudy
May 1	52	61	49	·64	0	Rain
2	55	60	51	·72	26	Showery
3	50	59	55	30·00	37	Cloudy
4	52	63	54	29·90	49	Fair
5	53	60	46	·80	54	Stormy
6	46	55	47	·99	10	Small rain
7	48	52	50	·70	0	Rain
8	47	54	48	·58	0	Rain
9	49	56	50	·50	0	Rain
10	53	64	53	·70	42	Fair
11	54	67	56	·76	54	Fair
12	57	77	64	·66	61	Fair
13	64	74	66	·50	70	Fair
14	66	64	50	·66	66	Fair
15	50	68	56	·76	60	Fair
16	60	66	50	·78	40	Fair
17	52	66	50	·85	85	Fair
18	52	67	56	·87	66	Fair
19	54	54	48	·89	0	Rain
20	53	69	56	·80	82	Fair
21	55	64	57	·67	26	Cloudy
22	57	69	60	·65	62	Cloudy, thunder in the evening
23	56	67	51	·85	62	Fair
24	57	68	57	·85	42	Showery
25	61	71	60	30·00	50	Fair
26	68	84	67	30·00	76	Fair

N. B. The Barometer's height is taken at one o'clock.

LXIX. *The Bakerian Lecture. On some of the Combinations of Oxymuriatic Gas and Oxygen, and on the chemical Relations of these Principles to inflammable Bodies.* By Humphry Davy, *Esq. LL.D. Sec. R.S. F.R.S. E. M.R.I.A. and M.R.I.**

1. *Introduction.*

In the last communication which I had the honour of presenting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body improperly called in the modern nomenclature of chemistry, *oxymuriatic acid gas*, has not as yet been decompounded; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxygen gas.

My objects in the present lecture, are to detail a number of experiments which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxygen; and likewise to present some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the Society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects important both in their connexion with the higher departments of chemical philosophy, and with the œconomical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

2. *On the Combinations of Oxymuriatic Gas and Oxygen with the Metals from the fixed Alkalies.*

The intensity of the attraction of potassium for oxymuriatic gas, is shown by its spontaneous inflammation in that substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such that one grain of potassium absorbs about 1·1 cubical inch of oxymuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in

* From the Philosophical Transactions for 1811, Part I.

the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium; the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannot be made to inflame, without heat in oxymuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oxymuriatic gas be used, not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium and pure oxymuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash that has undergone ignition.

The combustion of potassium and sodium in oxygen gas is much less vivid than in oxymuriatic gas. From this phænomenon, and from some others, I was inclined to believe that the attraction of these metals for oxygen is feebler than their attraction for oxymuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more fully than I have yet attempted, the nature of the combinations of potassium and sodium with oxygen, and of potash and soda with water.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxygen gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina; and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxygen the platina might have absorbed; and except in cases when this precaution was taken, I found the absorption of oxygen much greater than could be accounted for by the production of the alkalies. In all cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish-brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air,—phænomena, which, at an early period of the inquiry, induced me to suppose that they were prot-oxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated

with

with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Since that time, these oxides, as I find by a notice in the *Moniteur* for July 5, 1810, have occupied the attention of MM. Gay Lussac and Thenard, and these able chemists have discovered that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxygen as potash, and the other 1·5 times as much as soda.

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately the quantity of oxygen contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash that had been fused; but in this case, though I am inclined to believe that some alkali was formed at the same time with the peroxides, yet I obtained an absorption of 2·6 cubical inches, in a case when 2 grains of potassium were employed, and of 1·63 cubical inches, in a case when a grain of sodium was used; but in this last instance the edge of the platina tray had been acted upon by the metal, and was oxidated*. The mercury in the barometer in these experiments stood at 30·12 inches, and that in the thermometer at 62° Fahrenheit.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxygen, as MM. Gay Lussac and Thenard state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound that did not redden paper tinged with turmeric, without the addition of water.

When potassium is brought in contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogen is produced, and the oxide of potassium

* MM. Gay Lussac and Thenard have stated in the paper above referred to, that common potash and barytes absorb oxygen when heated. It would seem that the action of the fixed alkalies, and of barytes, on platina, depends on the production of the peroxides. I have little doubt but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiments.

 formed.

formed. I thought that by ascertaining the quantity of nitrogen evolved by the action of a given weight of potassium, and comparing this with the quantity of oxygen disengaged from the oxide by water, I might be able to determine its composition accurately. A grain of potassium acting in this way, I found produced only $\frac{16}{100}$ of nitrogen; and the red oxide, by its action upon water, produced less than half a cubical inch of oxygen, so that it is probable that potash as well as its peroxide is formed in the operation.

Sodium, when brought in contact with fused nitre, produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of $\frac{1}{2}$ a grain of sodium; but it appeared that no peroxide had formed, for the mass gave no oxygen by the action of water.

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

A grain of potassium was gently heated in a small green glass retort containing oxygen; it burnt slowly, and with a feeble flame; a quantity of oxygen was absorbed equal to $\frac{90}{100}$ of a cubical inch; by heating the retort to dull redness, oxygen was expelled equal to $\frac{38}{100}$ of a cubical inch; the mercury in the thermometer in this experiment stood at 63° Fahrenheit, and that in the barometer at 30·1 inches.

In experiments on the electrical decomposition of potash and soda, when the Voltaic battery employed contains from 500 to 1000 series in full action, the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of M. Ritter, that these bodies may be produced likewise in Voltaic operations on potash, at the positive surface.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the Voltaic circuit, as well as those produced by the exposure of the metals to heat and air, as prot-oxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that prot-oxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half of their weight of potash and soda that have been ignited, become first of a bright azure, then produce a considerable quantity of hydrogen, and at last form

form a gray coherent mass, not fusible at a dull red heat, and which gives hydrogen by the action of water.

Whether these are true prot-oxides, or merely mixtures of the alkaline metals with the alkalies, or with the alkalies and reduced silex from the glass, I shall not at present attempt to decide.

Potassium, I find, heated in a similar manner with fused potash, in a tube of platina, gives, after having been ignited, a dark mass that effervesces with water; but even in this case it may be said that the alloy of platina and potassium interferes, and that the substance is not a prot-oxide, but merely dry alkali mixed with this alloy.

As the pure alkalies were unknown till the discovery of potassium and sodium*, and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

When potassium and sodium are burnt in oxygen gas upon platina, and heated to redness to decompose the per-oxide of potassium, the alkalies are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly by a still further increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties they resemble the alkalies formed upon metallic substances; and are distinguished in a marked manner by their difficult fusibility from the potash and soda prepared by alcohol.

M. D'Arcet and more distinctly M. Berthollet have con-

* Stahl approached nearly to the discovery of the pure alkalies. He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim. Bech.* part ii. page 255. He procured caustic alkali also, by decomposing nitre by the metals. *Id.* p. 253.

I find that when nitre is decomposed in a crucible of platina, by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash which comes over in the process for procuring potassium by the gun-barrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

 cluded

cluded that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which M. Berthollet has rated at 13·9 per cent. for potash, and M. D'Arcet at 27 or 28 for potash, and 28 or 29 for soda*.

I have stated in the last Bakerian Lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed by adding oxygen to potassium as a standard.

The experiment from which I drew my conclusions, was made on the action of silex and potash fused together, and I regarded the loss of weight as the indication of the quantity of moisture.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalies, and this appeared necessary for the complete elucidation of the subject.

I heated together in a green glass retort, 40 grains of potash, (that had been ignited for several minutes,) and 100 grains of boracic acid which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost $6\frac{1}{2}$ grains, and the receiver had gained 5·8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated it did not leave an appreciable quantity of residuum.

A similar experiment made upon soda heated to redness, but in which the water collected was not weighed, indicated 22·9 of water in 100 parts of soda.

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments show that this cannot be the case in any sensible degree.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited

* *Annales de Chimie;* tome Lxviii. p. 190.

the

the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide I added to it dry boracic acid, and heated the mixture to redness. Sub-borate of potash was formed, and there was not the slightest indication of the presence of moisture *.

It is evident from this chain of facts, that common potash and soda are hydrats, and the bodies formed by the combustion of the alkaline metals are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water †.

I shall

* These processes must not however be considered as showing that boracic acid that has been heated to whiteness is entirely free from water; they merely prove that such an acid gives off no water by combination with pure potash at a *red heat*. I have found that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hydrogen; when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar: by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend John George Children, Esq. and in which Mr. Children was so kind as to co-operate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55·5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22·2 grains; 80 grains of boracic glass were added to this mixture; a fresh effervescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases, is shown by the experiments of M. Berthollet, *Mem. d'Arcueil*, tom. ii. page 47. Indeed it is impossible to say that a neutral compound, or a fixed acid, is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would appear to be from 19 to 20 per cent. in the first, and from 23 to 25 in the second.

† After the experiments detailed in my two last papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers who have closely followed the progress of science, to offer any new evidences to prove that potassium and sodium are not hydrurets of potash and soda, particularly as MM. Gay Lussac and Thenard, the ingenious advocates of this notion, have acknowledged, in the *Moniteur* to which I have before referred, that it is not tenable; but on a subject so intimately connected

nected

I shall now resume the detail of the experiments that I have made, on the relative attractions of oxymuriatic gas and oxygen for the metals of the fixed alkalies. I burnt a grain of potassium in oxygen gas, in a retort of green glass, furnished with a stop-cock, and heated the oxide formed, to redness, to convert it into potash: half a cubical inch of oxygen was absorbed. The retort was exhausted, and very pure oxymuriatic gas admitted. The colour of the potash instantly became white, and by a gentle heat the whole was converted into muriate of potash: a cubical inch and $\frac{1}{8}$ of oxymuriatic gas were absorbed, and exactly half a cubical inch of oxygen generated. The barometer during this operation was at 30·3, the thermometer at 62 Fahrenheit. I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its admission into the retort, which rendered the absorption of oxygen a little less than it ought to have been. In the process it was heated

nected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. Dalton, in the second volume of the work which he entitles "*A New System of Chemical Philosophy*," of which he has had the goodness to send me a copy, has, I find, in his first pages, adopted the idea that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hydrogen. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxygen is procured from this by Voltaic electricity at one surface, and potassium at the other surface; Mr. Dalton, conceiving that this oxygen arises from the water, states that the hydrogen of the water must combine with the potash to form potassium. It is evident, that adopting such a plan of reasoning, lead or copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxygen is produced at the positive surface, and no hydrogen at the negative surface.

In my first experiments for producing potassium and sodium, I used a weak power, and in these instances procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hydrogen and sometimes of potassuretted hydrogen, connected with the formation of the metal.

Potassium brought in contact with red hot hydrat of potash, disengages abundance of hydrogen, and the whole is converted into difficultly fusible potash.

327 grains of hydrat of potash that had been ignited, were made to act in a gun-barrrel on 745 grains of iron turnings heated to whiteness. Some hydrogen was lost, and some hydrat of potash remained undecompounded, yet

heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oxymuriatic gas separated water as well as oxygen.

The phænomenon of the separation of water from hydrat of potash by oxymuriatic gas, was happily exemplified in an experiment in which I introduced oxymuriatic gas to the peroxide of potassium, formed in a large retort, and in which the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opake. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oxymuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxygen evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed, so that it is scarcely possible to doubt that all the hydrogen produced from the decomposed hydrat of potash was liberated.

Mr. Dalton conceives that there is an analogy between potassium and sodium, and the compounds of hydrogen with sulphur, phosphorus, and arsenic; but I am at loss to trace any similarity between sulphuretted hydrogen, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. Dalton considers the volatility of potassium and sodium as favouring the idea of their containing hydrogen; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by M. Ritter; but it may not be amiss to add, that if potassium is a compound of hydrogen and potash, hydrat of potash must contain an equal quantity of hydrogen, with the addition of a light gaseous element, oxygen, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. Dalton states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas and nitrous oxide: this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxygen and some of the nitrogen from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogen and its excess of oxygen, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

MM. Gay Lussac and Thenard have convinced themselves that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

When

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oxymuriate of potassium *. I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation of the nature of the mutual decomposition of this substance and hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{13}{16}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

In the few experiments which I have made on the action of sodium and soda on oxymuriatic gas, the phænomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oxymuriatic gas as potassium.

When common salt that has been ignited is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat pure sodium is obtained; and this process affords an easy mode, and the one I have always lately adopted for procuring that metal. No hydrogen is disengaged in this operation, and two parts of potassium I find produce rather more than one of sodium.

From the series of proportions that I have communicated in my last paper, it is evident that 1 grain of potassium ought to absorb 1·08 cubical inch of oxymuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2·16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian lecture, is 25·4 of oxygen to 74·6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22· †; from which it is evident, that a grain of sodium

* *i. e.* muriate of potash.

† Or if soda be considered as deutoxide, which seems probable from the experiments before detailed; and on this supposition, the salts of soda must

dium ought to absorb nearly 2 cubical inches of oxymuriatic gas, and that the same quantity converted into soda, would

must be conceived to contain double proportions of acid. On either datum the proportion of oxygen in water must be taken as 7·5, and that of hydrogen as 1, though other numbers might be found as divisors or multiples of those which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. Dalton as the original author of the hypothesis. that water consists of one particle of oxygen, and one of hydrogen; but I have since found that this opinion is advanced, in a work published in 1789, *A comparative View of the Phlogistic and Antiphlogistic Theories*, by William Higgins. In this elaborate and ingenious performance, Mr. Higgins has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxygen, and one of nitrogen. Mr. Higgins had likewise drawn the just conclusion respecting the constitution of sulphuretted hydrogen, from its electrical decomposition. As hydrogen is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea the proportions in ammonia will be three of hydrogen to one of nitrogen, and the number representing the smallest proportion in which nitrogen is known to combine will be 13·4. Mr. Dalton, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4·7 or 5·1, as the number representing the weight of the *atom* of nitrogen; and has quoted my experiments, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the inquiries on nitric acid, nitrous gas, nitrous oxide, and on the decomposition of nitrate of ammonia stated in that work, conform much more nearly to the number 13·4.

According to Mr. Dalton, nitrate of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. Dalton estimates the quantity of water in nitric acid of specific gravity 1·54, at 27·5 per cent.; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent. of water; and one quantity of sulphuric acid, according to him, will produce from nitre more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgement and candour for a correction of his views.

It is impossible not to admire the ingenuity and talent with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by Richter and Guyton de Morveau, in the mutual decompositions of the compounds of hydrogen and nitrogen, of nitrogen and oxygen, of water and the oxymuriatic compounds; in the multiples of oxygen in the nitrous compounds; and those of acids in salts, observed by Drs Wollaston and Thomson; and above all, in the decompositions by the Voltaic apparatus. Where oxygen and hydrogen, oxygen and inflammable bodies, acids and alkalies, &c. must separate in uniform ratios.

decompose

decompose nearly 4 cubical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium 22·, and one of oxymuriatic gas 32·9; and this estimation is very near that which may be gained from Dr. Marcet's analysis of this substance. Hydrat of potash ought to consist of one proportion of potash, represented by 48·, and one of water, represented by 8·5. This gives its composition as 15·1 of water, and 84·9 of potash. Hydrat of soda ought, according to theory, to contain one proportion of soda 29·5, and one of water 8·5, which will give in 100 parts 22·4 of water; and the experiments that I have detailed, conform as well as can be expected with these conclusions.

The proportions of potash and soda, indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analysis, particularly those of M. Berthollet; or the differences are such as admit of an easy explanation.

I stated in my last communication, the probability that the oxygen in the hyper-oxymuriate of potash was in triple combination with the metal and oxymuriatic gas; and new facts respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxygen, would probably contain six proportions; for, according to Mr. Chenevix's analysis, which is confirmed by one made in the laboratory of the Royal Institution, by Mr. E. Davy, hyper-oxymuriate of potash must consist of 40·5 potassium, 32·9 oxymuriatic gas, and 45 of oxygen.

I have mentioned, that by strongly heating the peroxide of potassium in oxymuriatic acid, all the oxygen is expelled, and a mere combination of oxymuriatic gas and potassium formed. I thought it possible, that at a low temperature, a combination might be effected, and I have reason to believe that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oxymuriatic gas which was absorbed: some oxygen was expelled on the fusion of the peroxide, but a salt remained, which gave oxymuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

It seems evident, that in the formation of the hyperoxymuriate of potash, one quantity of potash is decomposed by the attraction of oxymuriatic gas to form muriate of potash; but the oxygen, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oxymuriatic gas.

The proportions required for these changes may be easily deduced

deduced from the data which have been stated in the preceding pages. Five proportions of potash, equal to 240 grains, must be decomposed to form with an equal number of proportions of oxymuriatic gas equal to 164·5 grains, five proportions of muriate of potash equal to 367 grains; and five of oxygen equal to 37·5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oxymuriatic gas equal to 32·9, to form one proportion, equal to 118·4 grains of hyperoxymuriate of potash.

3. *On the Combinations of the Metals of the Earths with Oxygen and Oxymuriatic Gas.*

The muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple attractions: thus, they are not altered by dry boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths.

From this circumstance, I was induced to believe that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oxymuriatic gas; and such experiments as I have been able to make, confirm the conclusion.

When baryta, strontia, or lime, is heated in oxymuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxygen is expelled from the earth. I have never been able to effect so complete a decomposition of these earths by oxymuriatic gas, as to ascertain the quantity of oxygen produced from a given quantity of earth. But in three experiments made with great care I found that one of oxygen was evolved for every two in volume of oxymuriatic gas absorbed.

I have not yet tried the experiment of acting upon oxymuriatic gas by the bases of the alkaline earths; but I have not the least doubt that these bodies would combine directly with that substance, and form dry muriates.

In the last experiments that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed, by exposing the residuum of amalgams to the air. I found that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from crystals of the earth, as M. Berthollet has shown, is a fusible hydrat, and I found that this earth gave moisture when decomposed by oxymuriatic gas; and the lime, in hydrat of lime, was much more rapidly decomposed

composed by oxymuriatic gas than quicklime, its oxygen being rapidly expelled with the water.

Some dry quicklime was heated in a retort, filled with muriatic acid gas; water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hydrogen of the acid combining with the oxygen of the lime.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark-coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassium had all disappeared, and the retort had received a heat at which potassium entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oxymuriatic gas in these processes, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combinations of muriatic acid gas with magnesia, alumine and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free. I conjectured from this circumstance, that oxymuriatic gas would not expel oxygen from these earths, and the suspicion was confirmed by experiments. I heated magnesia, alumine, and silex to redness in oxymuriatic gas, but no change took place.

MM. Gay Lussac and Thenard have shown that baryta is capable of absorbing oxygen; and it seems likely, (as according to Mr. Chenevix's experiments, most of the earths are capable of becoming hyper-oxymuriates) that peroxides of their bases must exist.

I endeavoured to combine lime with more oxygen, by heating it in hyper oxymuriate of potash, but without success; at least after this process it gave off no oxygen in combining with water. The salt called oxymuriate of lime, made for the use of the bleachers, I found gave off oxygen by heat, and formed muriate of lime.

From the proportions which I have given in the last Bakerian

Bakerian lecture, but which were calculated from the analyses of sulphates, it follows that if the muriates of baryta, strontia, and lime, be regarded as containing one proportion of oxymuriatic gas, and one of metal, then they would consist of 71* barium, 46 strontium, and 21 calcium, to 32·9 of oxymuriatic gas.

To determine how far these numbers are accurate, fifty grains of each of these muriates that had been heated to whiteness were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

From experiments to be detailed in the next section, it appears that horn-silver consists of 12 of silver to 3·9 of oxymuriatic gas, and consequently that barium should be represented by 65·1, strontium by 46·1, and calcium by 20·8.

4. *On the Combinations of the Common Metals with Oxygen and Oxymuriatic Gas.*

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments that I have made on the combinations of oxymuriatic gas with metals; I must confine myself to a general statement of the mode of operating, and the results. I used in all cases small retorts of green glass, containing from three to six cubical inches, furnished with stop-cocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by means of a spirit-lamp, and after cooling, the results were examined, and the residual gas analysed.

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oxymuriatic gas, and the volatile metals with flame. Arsenic, antimony, tellurium and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and iron and copper to redness; tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

The product from arsenic was butter of arsenic; a dense,

* If Mr James Thomson's analysis of sulphate of barytes be made the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65·5.

limpid,

limpid, highly volatile fluid, a non-conductor of electricity, and of high specific gravity, and which when decomposed by water, gave oxide of arsenic and muriatic acid. That from antimony, was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexahedral plates, and giving, by its decomposition by water, white oxide.

The product from tellurium, in its sensible qualities, resembled that from antimony, and gave when acted on by water white oxide.

The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

The combination of oxymuriatic gas and iron was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semi-transparent on cooling, and which gave a green fluid, and a green precipitate by the action of water*.

The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown: a muriate of manganese, which did not redden litmus, remained in solution; and an insoluble matter remained of a chocolate colour†.

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungsten.

Tin afforded Libavius's liquor, which gave a muriate by

* It is worth inquiry, whether the precipitate from oxymuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystallized muriate of Peru. This last I find affords muriatic acid and water by heat.

The *resin of copper* discovered by Boyle, formed by heating copper with corrosive sublimate, probably contains only one proportion of oxymuriatic gas, whilst that above referred to must contain two.

† When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues.

the

the action of water containing the oxide of tin, at the maximum of oxidation.

Silver and lead produced horn-silver and horn-lead, and bismuth, butter of bismuth. The absorption of oxymuriatic gas was in the following proportions for two grains of each of the metals: for arsenic 3·6 cubical inches, for antimony 3·1, for tellurium 2·4, for mercury 1·5 *, for zinc 3·2, for iron 5·8, for tin 4, for bismuth 1·5, for copper 3·4, for lead ·9, for silver, the absorption of volume was $\frac{9}{10}$, and the increase of weight of the silver was equivalent to $\frac{6}{10}$ of a grain †.

In acting upon metallic oxides by oxymuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals more readily than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red reat, whilst the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, whilst the white oxide readily decomposed.

In cases where oxygen was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus two grains of red oxide of mercury absorbed $\frac{9}{10}$ of a cubical inch of oxymuriatic gas, and afforded 0·45 of oxygen ‡. Two grains of dark olive oxide from

* The gas in these experiments was not freed from aqueous vapour, and as stop-cocks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oxymuriates. The processes on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30·26 inches to 30·15, and the height of that in the thermometer from 63·5 to 61 Fahrenheit.

The experiment on silver was made at the temperature of 52 Fahrenheit, and under a pressure equal to that of 29·9 inches.

† This agrees nearly with another experiment made by my brother, Mr. John Davy, in which 12 grains of silver increased to 15·9 during their conversion into horn-silver.

‡ I have made two analyses of corrosive sublimate and calomel, with considerable care. I decomposed 100 grains of corrosive sublimate, by 90 grains of hydrat of potash. This afforded 79·5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9·15 cubical inches of oxygen gas; the muriate of silver formed from the 100 grains was 102·5.

100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive-coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4·3 cubical inches of oxygen. The quantity of horn-silver formed from the 100 grains was 58·75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate

from calomel decomposed by potash, absorbed about $\frac{94}{100}$ of oxymuriatic gas, and afforded $\frac{24}{100}$ of oxygen, and corrosive sublimate was produced in both cases.

In the decomposition of the white oxide of zinc, oxygen was expelled exactly equal to half the volume of the oxymuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxygen was given off in either case, but butter of arsenic and arsenical acid formed in one instance, and the ferruginous sublimate and red oxide of iron in the other.

Two grains of white oxide of arsenic absorbed 0·8 of oxymuriatic gas *.

I doubt not that the same phænomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oxymuriatic gas, and when it is susceptible of different degrees of oxidation, and in which the peroxide is used.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn-coloured oxide of tin; water rapidly separated, and Libavius's liquor was formed.

From the proportions which may be gained in considering the volumes of oxymuriatic gas absorbed by the different metals, in their relations to the quantity of oxygen which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oxymuriatic gas combine

sublimate was 78·7; the quantity of muriate of silver formed was 103·4; the oxide produced from calomel weighed 88 grains; the horn-silver formed was 57½ grains. I am inclined to put most confidence in the last analyses; but the tenor of both is to show that the quantity of oxymuriatic gas in corrosive sublimate is exactly double that in calomel, and that the orange oxide contains twice as much oxygen as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel is owing to a slight admixture of orange oxide, formed by the oxygen of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxygen. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been over-rated.

* A singular instance of the tendency of the oxide of arsenic to become arsenical acid, occurs in its action on fused hydrat of potash; the water in the hydrat is rapidly decomposed, and arseniuretted hydrogen evolved, and arseniate of potash formed.

combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds*.

5. *General Conclusions and Observations, illustrated by Experiments.*

All the conclusions which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new inquiries.

Oxymuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxygen, or causes it to enter into new combinations.

If it be said that the oxygen arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas?

If there existed any acid matter in oxymuriatic gas, combined with oxygen, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oxymuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act under common circumstances on fixed alkaline bases, such as dry lime or magnesia. Oxymuriatic gas, like oxygen, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hydrogen, it instantly reddens the driest litmus paper, though a gaseous body. Contrary to acids, it expels oxygen from prot-oxides, and combines with peroxides.

When potassium is burnt in oxymuriatic gas, a dry compound is obtained. If potassium combined with oxygen is employed, the whole of the oxygen is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxygen is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

* From the experiments detailed in the note in the opposite page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear from the results, page 417, about 100. The numbers of other metals may be learnt from the data in the same page, but from what has been stated, these data cannot be considered as very correct.

An argument in favour of the existence of oxygen in oxymuriatic gas, may be derived by some persons from the circumstances of its formation, by the action of muriatic acid on peroxides, or on hyper-oxymuriate of potash; but a minute investigation of the subject will, I doubt not, show that the phænomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oxymuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oxymuriatic gas and hydrogen, there is no simple explanation of the result, except by saying that the hydrogen of the muriatic acid combined with oxygen from the peroxide to produce water.

Scheele explained the bleaching powers of the oxymuriatic gas, by supposing that it destroyed colours by combining with phlogiston. Berthollet considered it as acting by supplying oxygen. I have made an experiment, which seems to prove that the pure gas is incapable of altering vegetable colours, and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxygen.

I filled a glass globe containing dry powdered muriate of lime, with oxymuriatic gas. I introduced some dry paper tinged with litmus that had been just heated, into another globe containing dry muriate of lime; after some time this globe was exhausted, and then connected with the globe containing the oxymuriatic gas, and by an appropriate set of stop-cocks the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white *.

Paper that had not been previously dried, brought in contact with dried gas, underwent the same change, but more slowly.

The hyper-oxymuriates seem to owe their bleaching powers entirely to their loosely combined oxygen; there is a strong tendency in the metal of those in common use, to form simple combinations with oxymuriatic gas, and the oxygen is easily expelled or attracted from them.

* The last experiments were made in the laboratory of the Dublin Society; most of the preceding ones in the laboratory of the Royal Institution; and I have been permitted to refer to them by the managers of that useful public establishment.

It

It is generally stated in chemical books, that oxymuriatic gas is capable of being condensed and crystallized at a low temperature; I have found by several experiments that this is not the case. The solution of oxymuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of Fahrenheit. The mistake seems to have arisen from the exposure of the gas to cold in bottles containing moisture.

I attempted to decompose boracic and phosphoric acids by oxymuriatic gas, but without success; from which it seems probable, that the attractions of boracium and phosphorus for oxygen are stronger than for oxymuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold seem to have a stronger attraction for oxymuriatic gas than for oxygen.

I have as yet been able to make very few experiments on the combinations of the oxymuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted and the sulphuretted liquors unite with each other, and with the liquor of Libavius, but without any remarkable phænomena.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oxymuriate of phosphorus, through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded, by the action of water, muriate and phosphate of lime.

I introduced some vapour from the heated phosphoric sublimate, into an exhausted retort containing dry paper tinged with litmus: the colour slowly changed to pale red. This fact seems in favour of the idea that the substance is an acid; but as some minute quantity of aqueous vapour might have been present in the receiver, the experiment cannot be regarded as decisive: the strength of its attraction for ammonia is perhaps likewise in favour of this opinion. All the oxymuriates that I have tried, indeed, form triple compounds with this alkali; but phosphorus is expelled by a gentle heat from the other compounds of oxymuriatic gas and phosphorus with ammonia, and the substance remaining in combination is the phosphoric sublimate.

6. *Some Reflections on the Nomenclature of the Oxymuriatic Compounds.*

To call a body which is not known to contain oxygen, and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but dephlogisticated marine acid is a term which can hardly be adopted in the present advanced æra of the science.

After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties—its colour, and to call it *Chlorine*, or *Chloric* gas*.

Should it hereafter be discovered to be compound, and even to contain oxygen, this name can imply no error, and cannot necessarily require a change.

Most of the salts which have been called muriates, are not known to contain any muriatic acid, or any oxygen. Thus Libavius's liquor, though converted into a muriate by water, contains only tin and oxymuriatic gas, and horn-silver seems incapable of being converted into a true muriate.

I venture to propose for the compounds of oxymuriatic gas and inflammable matter, the name of their bases, with the termination *ane*. Thus argentane may signify horn-silver; stannane, Libavius's liquor; antimonane, butter of antimony; sulphurane, Dr. Thomson's sulphuretted liquor; and so on for the rest.

In cases when the proportion is one quantity of oxymuriatic gas, and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter combine with one of gas; or two or more of gas, with one of inflammable matter, it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

* From χλωρος.

The

The name muriatic acid, as applied to the compound of hydrogen and oxymuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterized in the usual manner, and as the other neutral salts.

Thus muriate of ammonia and muriate of magnesia are perfectly correct expressions.

I shall not dwell any longer at present upon this subject. What I have advanced, I advance merely as suggestion, and principally for the purpose of calling the attention of philosophers to it*. As chemistry improves, many other alterations will be necessary; and it is to be hoped that, whenever they take place, they will be made independent of all speculative views, and that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

LXX. *On*

* It may be conceived that a name may be found for the oxymuriatic gas in some modification of its present appellation which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oxymuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oxymuriatic gas must be called demuriates or oxymuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend, for a time, the adoption of any new terms for these compounds. It is possible that oxymuriatic gas may be compound, and that this body and oxygen may contain some common principle; but at present we have no more right to say that oxymuriatic gas contains oxygen than to say that tin contains hydrogen; and names should express things and not opinions; and till a body is decompounded, it should be considered as simple.

In the last number of Mr. Nicholson's Journal, which appeared February 1st, whilst this sheet was correcting for the press, I have seen an ingenious paper, by Mr. Murray, of Edinburgh, in which he has attempted to show that oxymuriatic gas contains oxygen. His methods are, by detonating oxymuriatic gas in excess, with a mixture of hydrogen, and gaseous oxide of carbon, when he *supposes* carbonic acid is formed; and by mixing oxymuriatic gas in excess, with sulphuretted hydrogen, when he *supposes* sulphuric acid, or sulphureous acid is formed. In some experiments, in which my brother, Mr. John Davy, was so good as to co-operate, made over boiled mercury, we found, that seven parts of hydrogen, eight parts of gaseous oxide of carbon, and 20 parts of oxymuriatic gas, exploded by the electric spark, diminished to about 30 measures; and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water a gas remained, which equalled more than nine measures, and which was gaseous oxide of carbon, with no more impurity than might be expected from the air in the gases, and the nitrogen expelled from the ammonia; so that the oxygen in Mr. Murray's carbonic acid, it seems, was obtained from *water*, or from the carbonic oxide. Sulphuretted hydrogen, added over dry mercury, to oxymuriatic gas in excess, inflamed in two or three experiments; muriatic acid gas containing the vapour of oxymuriate

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LXX. *On M.* De Luc's *Electric Column.* *By* Thomas Forster, *Esq.*

To Mr. Tilloch.

Sir, In consequence of Mr. De Luc's Electric Columns having been described in your Philosophical Magazine, I think it right to inform your readers of a circumstance relating to it, which may prove interesting. I have observed that the action of this column is materially influenced by the state of the atmosphere: your readers are acquainted with the manner in which two bells, attached to the *plus* and *minus* end of the column, are made to ring by means of an insulated conducting clapper being suspended between them, and with the circumstance of their having rung for many months together in an instrument of Mr. B. M. Forster's at Walthamstow. Now I have observed that they sometimes pulsate very *strong and regularly*, at other times *weak and regularly*, at others *strong and irregularly*, or with intervals of quiescence, and sometimes both *weak and irregularly;* and these variations seem to me to be connected with peculiarities in the electric state of the atmosphere. For I cannot perceive that there is any correspondence between the kind of action of this column and the state of the hygrometer, barometer, or thermometer; but there seems to be a connection between it and certain appearances of the clouds, the peculiarities of which are (according to the modern theory) caused chiefly by electricity: when the air is dry, with strong easterly winds, when the *cirrus* cloud ramifying about in all directions, and occasionally accompanied by the other modifications, continues

of sulphur, was formed, which, when neutralized by ammonia, gave muriate of ammonia, and a combination of ammonia and oxymuriate of sulphur.

When a mixture of oxymuriatic gas in excess, and sulphuretted hydrogen, was suffered to pass into the atmosphere, the smell was that of oxymuriate of sulphur: there was not the slightest indication of the presence of any sulphuric or sulphureous acid. If Mr. Murray had used ammonia, instead of water, for analysing his results, I do not think he would have concluded, that oxymuriatic gas is capable of decomposition by such methods.

I shall not, at present, enter upon a detail of other experiments which I have made on this subject, in co-operation with my brother, as it is his intention to refer to them, in an answer to Mr. Murray's paper.

I shall conclude, by saying, that this ingenious chemist has mistaken my views, in supposing them hypothetical; I merely state what I have seen, and what I have found. There *may* be oxygen in oxymuriatic gas; but I can find none. I repeated Mr. Murray's experiments with great interest; and their results, when *water* is excluded, entirely confirm all my ideas on the subject, and afford no support to the hypothetical ideas which he has laboured so zealously to defend.

for

for a long time unattended by rain, when the nights are clear, and small meteors, called falling stars, are numerous; when, I say, these circumstances happen together, I have observed that the bells of this column always ring with very irregular pulsations; and further, when rain succeeds such kind of weather, it commonly happens that their pulsations become *weak*, or *cease altogether, and the bells become silent:* on the contrary, when the weather is fair, and when only diurnal *cumuli* prevail, they usually pulsate *regularly*. An ingenious meteorologist suggested to me, the other day, that the irregular pulsation of the bells might be occasioned by the electric fluid's passing downward to the earth *in pulsations*, which might be the case when it was very irregularly distributed in the atmosphere.

To me it appears that this irregular distribution of the electric fluid would be indicated by the multiform appearance of the *cirrus* cloud which I have described, for the particular office of this cloud seems to be that of serving as a conductor of electricity. The same circumstance would also give rise to the occasional appearance of other modifications. All however that can be said on this interesting subject at present is, *that there seems to be a connection worth attending to between the kind of action of the column, and the kind of weather which prevails, indicated by the various and peculiar appearances of the clouds.* Future observations may lead to the knowledge of adjunct circumstances which may have their share in producing these changes. To engage the co-operation of other meteorologists, by which alone the science can be brought to any degree of perfection, is the object I have in view in soliciting the favour that this may be inserted in your Philosophical Magazine. I remain, sir,

Your constant reader and servant,

St. Helen's Place, June 14, 1811. THOMAS FORSTER.

LXXI. *Memoir on the different Compounds formed by the Re-action of Sulphuric Acid on Camphor.* By M. CHEVREUL*.

HAVING shown in my two former memoirs, that tanning substances, formed by the re-action of nitric acid upon many vegetable compounds, were combinations of that acid with different substances, the greater part of which are unknown

* *Annales de Chimie*, tome lxx.

to us; it remains for me to inquire, if the third species of *tannin* of Mr. Hatchett (formed by the sulphuric acid) is a combination analogous to the others. I directed my researches to the tanning matter obtained from camphor, because it is that which particularly engaged the attention of Mr. Hatchett. Although I do not think that all the immediate principles of vegetables, when brought into contact with sulphuric acid, would be acted upon precisely in the same manner as camphor, I yet have reason to believe, that the results presented by this last are applicable to those substances which have a similar composition. The sulphuric acid, in its mode of acting, appears in general to possess more uniformity than the nitric acid does. The experiments which I am going to relate, were made upon camphor that was perfectly pure.

I put into a retort, to which a balloon was adapted, thirty grammes of camphor and sixty of pure sulphuric acid. The mixture became dark and yellow. I exposed it to a gentle heat for two hours. A great deal of sulphurous acid was disengaged. I then poured upon the thick brown liquor remaining in the retort, sixty grammes of sulphuric acid, and distilled. There came over into the receiver weak sulphuric acid, sulphurous acid, and a *yellow volatile oil* having a strong smell of camphor. Towards the end of the operation there was produced a small quantity of sulphuretted hydrogen, which was decomposed by the sulphurous acid; there was then scarcely any liquor left in the retort.

The residue, after distillation, was washed in boiling water until the washings were no longer sensibly coloured. The first washings contained an excess of sulphuric acid; the others scarcely contained enough to be sensible to barytes. I shall first begin with the examination of the *coaly residuum*, and shall afterwards treat of the matters contained in the washings.

1. *Action of Water.*

The coaly residuum was of a bright black colour; its taste was a little acid, upon its being held some time in the mouth; it slightly reddened turnsole paper moistened with water. When boiled some hours in distilled water, the water was scarcely at all tinged. The water thus boiled upon the matter, formed no precipitate with nitrate of barytes and acetate of lead, even after standing twenty-four hours. Evaporated to dryness, there remained a trace of brownish colour, which became red on the application of barytes water. These trials convinced me there was no longer

longer any uncombined sulphurous acid in this coaly residuum, and that, if it contained any, it must have been in a state of close combination.

2. *Action of Heat.*

I distilled four grammes of the coaly residuum in a small retort provided with a balloon, and obtained the following products; 1. a little moisture; 2. sulphurous acid; 3. sulphuretted hydrogen, part of which was decomposed by the sulphurous acid, and part remained in the balloon, although there was in it an excess of the acid; 4. a red oil, which became brown on exposure to the air. This product, some time after its distillation, gave out an aromatic sulphurous odour resembling that of amber and pyritous coal; saturated with potass, it gave out volatile alkali, but in too small a quantity to enable me to draw any inference from its presence.

I observed in another experiment, made over mercury in a very small apparatus, that it gave out carbonic acid gas, oxy-carburetted hydrogen gas, sulphuretted hydrogen gas, and much less sulphurous acid than in the former distillation, which was made into a vessel containing a large quantity of atmospheric air. The action of heat proves, that besides carbon and hydrogen there are contained in the coaly residuum sulphur and oxygen, but it does not determine whether these last exist in it in the state of sulphuric acid, or are immediately combined with the carbon and hydrogen. However, if we consider that the coaly residuum reddens turnsole, the first opinion is the most probable, and I adopt this as being the one most consistent with the facts and with analogy *.

The matter remaining in the retort after the distillation of the remainder of the coaly substance was black, semifused, formed of small shining grains, and resembled coak; it weighed two grammes and two deci-grammes. It was inodorous; but, on being exposed to the air for some time on a plate of copper, it blackened that metal, and gave out a smell of sulphuretted hydrogen. This phænomenon induced me to make some experiments upon this substance, in order to ascertain in what state the sulphur existed in it. I boiled it in water, but this did not dissolve the least particle of it; it caused no precipitate either with water of barytes, or with a solution of acetate of lead. I boiled it several

* Yet it is not impossible but there may be in this compound a small quantity of sulphur in a combustible state.

several hours in a solution of potass, and the result was the same.

Not being able by these methods to detect the presence of sulphur in this substance, I thought of detonating it with nitrate of potass. The residuum of the detonation, dissolved in water, filtered and supersaturated with nitric acid, afforded by the addition of nitrate of barytes an abundant precipitate of the sulphate. These experiments show that sulphur may be so intimately combined with carbon as to resist the action of a red heat, and of liquid potass. We cannot admit the presence of sulphuric acid in this combination; for all the known facts prove that the compounds in which this acid is the most concentrated do not resist, at a high temperature, the affinities of carbon and hydrogen. If oxygen at all exist in this combination, it can only be in a very small proportion, since it does not by heat act upon the combustible substances, so as to form with them gaseous products. From all these considerations, it appears to me, that we ought to regard the matter remaining after the distillation of the coaly residuum as a combination of carbon, sulphur, and a small quantity of hydrogen.

The solid combination of carbon and sulphur is not a fact entirely unknown to chemists. MM. Clément and Desormes have spoken of it in their Memoir upon Carbon; since which time M. Amédée Berthollet has shown, that by passing sulphur in a state of vapour over charcoal, a certain portion of the sulphur becomes fixed in it. The analysis of gunpowder yields the same combination. When the nitrate of potass has been carried off by means of water, the residuum, which is not soluble, and which consists of charcoal and sulphur, parts but with a very small quantity of this latter, by the action of heat; and to this is owing the sulphuric acid that is formed when the residuum already heated in a close crucible comes to be burned. These interesting facts on the analysis of gunpowder were communicated to me by M. Proust, when I gave him an account of my labours.

M. Proust, much struck with the odour of sulphurous acid generally exhaled from pit-coals towards the end of their combustion, thought at first that the sulphur in them had formed with the carbon a combination analogous to what I have described; but having afterwards observed that these coals plunged into nitric acid burned to the last without exhaling the least odour of sulphurous acid, he at length concluded that the sulphur was not combined with carbon, but with iron. I mention this fact, because it appears

pears to me to be of great importance, as I shall show hereafter.

3. *Action of Potass.*

Having in a former experiment observed that a mixture of ten parts of saturated carbonated potass with two parts of the coaly residuum exhibited scarcely the slightest traces of sulphuric acid, I wished to know if *pure* potass would have any greater effect. For this purpose, I boiled for two hours two grammes of the coaly residuum in water containing six grammes of pure potass dissolved in it. I left the substances to react on each other for the space of 12 hours afterwards. I then poured off the water and filtered it. A brown liquid ran through, and there remained upon the paper a black matter, which was washed in boiling water until the water was no longer coloured, and no longer gave any precipitate with water of barytes. The filtered alkaline solution was saturated with nitric acid*, there was a slight effervescence, a precipitation of *brown floccules* took place, and the liquid lost its colour. This liquor being again filtered, there was added to it a solution of barytes; at first there was not any precipitation, but in about two hours it threw down a very small particle of sulphate. Hence it follows, that the potass subtracts from the coaly residuum but an infinitely small quantity of sulphuric acid.

The *brown floccules*, which had been precipitated from the alkaline liquor by means of the nitric acid, were washed, first in cold and afterwards in warm water: this last dissolved a portion of them, and without doubt would have dissolved the whole, had it been in sufficient quantity. This solution was slightly acid: sulphuric, nitric, and muriatic acids caused a precipitation in it, probably by uniting with the matter it held in solution; barytes produced from it a precipitate soluble in hot nitric acid. I had too small a quantity of the brown floccules dissolved in potass to determine the nature of them, but I believe them to be composed of coaly residuum, nitric acid, and a small quantity of potass.

Part of the coaly residuum which had not been dissolved in the alkali, being well washed and dried, gave out when heated in a glass vessel, 1. carbonic acid gas in great quantity; 2. sulphuretted hydrogen gas; 3. oxy-carburetted hydrogen gas; 4. a coal which gave out a very strong smell of sulphur when breathed upon. I supposed from this cir-

* An excess of acid is necessary to render the precipitation complete. It would appear that this excess enters into combination.

cumstance

cumstance, that it might retain some sulphuret of potass, and I washed it in boiling water; but if the water dissolved any of it, it was too small a quantity to be rendered sensible by the acetate of lead, which did not blacken the liquor. After these results, I adopted another method of analysis; I burned the coal and obtained a grayish cinder, which being washed in water afforded some carbonate and some sulphate of potass, discoverable by the solutions of barytes and of platina. As, therefore, there were in this compound some sulphur and some potass, not soluble in hot water, it is very natural to conclude, that these substances are fixed in the coaly matter by a real chemical affinity. From all these circumstances, I am of opinion, that one part of the alkaline and earthy bases found in the coals of wood, &c. may exist in them in a state of strict combination.

4. *Action of Nitric Acid.*

Five grammes of the coaly residuum were put into a retort with fifty grammes of nitric acid heated to 32°*. Red vapours and carbonic acid were immediately disengaged. On applying heat, the action became much stronger, and the liquor foamed greatly. When the greatest part of the liquid had passed over into the receiver, I poured twenty-five grammes more of the acid into the retort: a complete solution took place, and the liquor was of a deep yellow orange colour: when concentrated in the retort, I poured it into a glass of water; there was precipitated a deep yellow flocky matter; this was separated by the filter, and washed with a large quantity of cold water. All the waters were mixed and evaporated to dryness, so as to drive off in a great measure the excess of acid. The residue was treated by a smaller quantity of water; by these means, there was procured a small portion of yellow floccose matter, partly soluble. I first examined the matter dissolved in the water, and then the yellow flocky substance.

The aqueous solution, separated from its excess of acid, and from the matter partly soluble, by several successive evaporations and solutions, was of a brown-yellow colour; its taste was acid, bitter, and astringent; yellow floccules were precipitated from it by gelatin. It contained a little sulphuric acid, discoverable by barytes; it is true, there was a flaky precipitate formed by the astringent substance, but this was soluble in nitric acid. (I believe that this sul-

* Centigrade Thermometer.

phuric

phuric acid proceeded from a portion of the coaly residuum, which had been completely destroyed by the nitric acid; for out of four experiments I made, two of them afforded me not the least trace of sulphuric acid; the precipitate formed by barytes was entirely soluble in nitric acid.) In order to separate the uncombined sulphuric acid, I boiled the astringent liquor which contained it with carbonate of barytes*; I then evaporated it to dryness, and treated the residue with water: there remained a small quantity of sulphate of barytes mixed with a little of the compound of astringent matter and barytes. The aqueous solution contained the astringent matter pure, or at least separated from the sulphuric acid. I evaporated this solution to dryness, and distilled the residue in a small glass retort with a balloon adapted to it. There came over, 1st, a yellow liquor of an acid and bitter taste,—I thought this was nitric acid; 2. sulphurous acid, which was ascertained by its odour, and by the combination it formed with oxide of copper; 3. sulphuretted hydrogen gas; 4. sulphur, proceeding from the decomposition of the sulphurous acid and sulphuretted hydrogen; 5. a thick brown oily liquor, mixed with prussic acid and ammonia; 6. some small needle-like crystals, whose nature I could not determine, on account of their small quantity; 7. a very puffy coal, containing ammonia, which was discoverable by the smell, and by a rod impregnated with weak nitric acid†. This coal contained a little sulphur; for, when burned with nitrate of potass, it afforded some sulphate by means of nitrate of barytes.

It follows from these facts, that the astringent matter, soluble in water, produced by the re-action of the nitric acid upon the coaly residuum, is formed of *nitric acid, sulphuric acid,* and the coaly matter.

I come now to the examination of the *yellow substance,* partly soluble in water. I boiled it in water until the washings no longer precipitated barytes in the form of sulphate. The matter thus washed was of an orange-yellow colour, it had an acid and bitter taste, it slightly reddened turnsole paper; heated in a glass tube, it quickly melted, disengaging an aromatic odour, a little sulphureous; boiled for several hours in water, it diffused a very strong smell of musk, and by distilling the water a liquor was obtained

* The liquor digested twenty-four hours upon litharge, equally lost its sulphuric acid, but the carbonate of barytes appeared to me preferable.

† I think this ammonia was formed when the hot coal came into contact with the air.

impregnated

impregnated with the same odour. The water which was boiled upon this substance was of a yellow colour and slightly acid, as appeared by the test of turnsole: with barytes water, it formed a precipitate of yellow floccules entirely soluble in nitric acid. Although this substance was boiled in a very large proportion of water, the latter was always tinged of a yellow colour. After being thus washed, it was subjected to the action of heat in a glass vessel; it melted, and there were obtained, among other products, nitrous vapour, nitrous gas, and a coal which had a sulphureous smell. Although I only operated on two decigrammes of this substance, I am led to believe that it is composed of *nitric acid, a small quantity of sulphuric acid, and of a carbonated matter more hydrogenated than that which forms the coaly residuum**.

The artificial production of a substance which possessed the odour of musk had already been observed by Geoffroy in 1726†, and by Margraff in 1758‡. The first discovered it upon mixing sulphuric and nitric acids with clear oil of petroleum; the second, in mixing rectified oil of amber with nitric acid. I myself have often had occasion to remark the same thing with the resinous substance which remains after the inflammation of oil of turpentine with the sulphuric and nitric acids. It appears, that in all these instances it is formed by the combination *of nitric acid, and sometimes of sulphuric acid, with the oily matter.*

The results of the action of nitric acid upon the coaly residuum offer the following interesting facts to our consideration.

1. They prove that the coaly residuum which contains sulphuric acid does not part with it upon being combined with nitric acid, (except the small quantity of it which is loose;) and that, if we rely upon the test of barytes to demonstrate the absence of sulphuric acid, we shall be deceived, because the precipitate in which we should suspect the acid to be would be soluble in nitric acid.

2. They confirm what I advanced in a former memoir, on the combination of sulphuric acid with the tanning matter of pit-coal, which forms with barytes a compound soluble in nitric acid.

3. They show that, by the re-action of nitric acid upon a coaly substance containing a great deal of hydrogen, a

* Having by chance left a certain quantity of this substance exposed to the rays of the sun, I observed at the end of two months that it was covered with small brilliant crystals.

† Memoirs of the Academy of Sciences. ‡ *Opuscules*, tome ii.

portion

portion of the hydrogen and of the carbon may combine with the nitric acid, and form a compound, fusible by heat, and partly soluble in water, and which possesses some of the properties of resins; and that, consequently, we are perhaps too hasty in regarding pit-coal, which furnishes similar products when treated with nitric acid, as being formed of *a resin* similar to that of recent vegetables, and of a coaly substance: this opinion may be true, but the facts brought in support of it are not sufficient to prove it.

4. They demonstrate, that, if the manner in which the coaly residuum is acted upon by the nitric acid indicates some analogy between this compound and pit-coal, yet the facts related above do not permit us, at least at present, to regard them as having one and the same origin. Indeed when the sulphuric acid acts upon the camphor, (and what takes place with that must take place with other vegetable substances,) it unites with the uncombined carbonated substance: consequently, if pit-coal was formed in the same manner, we must find sulphuric acid in it: besides, if we recollect that M. Proust, although he suspected there was a combination of sulphur with carbon in pit-coal, could not discover the least trace of sulphurous acid in that substance, when deprived of its sulphate of iron, we shall consider the hypothesis of the formation of pit-coal, by means of the sulphuric acid, as totally inadmissible.

I wished to determine the quantity of sulphuric acid contained in 100 parts of coaly residuum. For this purpose, I fused nitrate of potass in a crucible of platina, and projected into it the residuum in small quantities: although I had taken every necessary precaution that nothing should be lost, I always found that a small portion of the matter escaped combustion, and that it was carried out of the crucible by the gas, which was disengaged with impetuosity: the quantities of sulphuric acid here given, must not, therefore, be taken as completely accurate. In one experiment, I obtained a precipitate of sulphate of barytes, which indicated 5 parts of sulphuric acid; and, in another, a precipitate, which indicated 6 parts in the 100 of the coaly residuum.

One circumstance I must not omit to mention,—that I discovered traces of the existence of lime and oxide of iron in the camphor I employed in my experiments; and which had been twice sublimed.

Examination of the Washings.

These were, as I have mentioned, divided into two parcels;

cels; the first did not differ from the others, only in containing an excess of sulphuric acid. That this excess of acid might not decompose the vegetable matter in solution, I only reduced the liquor to one-fourth of its original quantity. In this state, it appeared green by reflection, and a yellowish red by refraction; (a phænomenon similar to what takes place in the solution of nephritic wood, with this difference, that the latter appears blue by reflection;) it precipitated gelatin, whence it is evident that it contained the tanning substance of Hatchett. To separate this latter from the sulphuric acid, I saturated it with barytes water. The sulphate was precipitated, and the combination of the astringent matter and barytes remained in the liquor: this was of a fine red colour: when muriatic acid was poured upon it to saturate the barytes, it regained the property of reflecting green, and parted with the rose colour: thus saturated, it precipitated gelatin; the combination of astringent matter and barytes, being dried and afterwards heated in a glass tube, was converted into a sulphuret.

The second parcels of water contained an exceedingly small quantity of sulphuric acid: to separate this, I poured into it a few drops of the combination I have mentioned, (*of the astringent matter and barytes*) and filtered it. The filtered liquor might be considered as a solution of pure astringent matter. It was acid, and presented the same appearances to the light as the first washing when concentrated; it precipitated gelatin; evaporated to dryness, there remained a substance, from which, when subjected to heat, there were disengaged sulphuretted hydrogen gas and sulphurous acid. The astringent matter had undergone some alteration by this evaporation; for, treated with water, it formed a solution which no longer reflected a green colour, and which precipitated barytes in brown floccules. This precipitate was soluble in nitric acid, and was converted into a sulphuret by calcination.

I regard the astringent matter as being composed of sulphuric acid, and a certain substance which at present is unknown to me. It is true, I have not been able directly to separate the sulphuric acid; but as I cannot detect in it either sulphuretted hydrogen or sulphurous acid, and as on the other hand it reddens turnsole, and contains oxygen and sulphur, the former opinion appears to me in the present state of science to be the most probable.

Recapitulation.

When sulphuric acid is distilled from camphor, there are obtained,

obtained, 1. *a volatile oil**, having the smell of camphor: 2. *a coaly residuum*, which is a combination of sulphuric acid, and highly hydrogenated carbon: 3. *an astringent matter*, which is also a combination of sulphuric acid, but which seems to differ from the former in two respects, viz. that the matter combined with the acid is more hydrogenated, and that the acid is in greater proportion.

The coaly residuum is not perceptibly soluble in water; it appears, however, to impart to it an exceedingly small quantity of astringent matter.

By distillation it affords sulphuretted hydrogen, sulphurous acid, and carbonic acid; the residuum is a combination of carbon and sulphur. This last combination is formed as often as sulphur meets with carbon strongly heated: thus, charcoal over which the vapour of sulphur has been passed; the mixture of charcoal and sulphur, (proceeding from the analysis of gunpowder,) when heated in a crucible, are compositions of this sort. It is very probable something of the same kind takes place in the decomposition of the sulphates by charcoal.

Potass takes up but a very inconsiderable portion of sulphuric acid from the coaly residuum: when these two substances are boiled together, two compounds are formed, one soluble, with an excess of alkali, and the other insoluble, with an excess of the coaly residuum. Nitric acid dissolves it totally, and forms with it two substances; one very soluble in water, which precipitates gelatin, and which *forms with barytes a compound soluble in nitric acid, although it contains sulphuric acid;* the other compound partly soluble, which fuses by heat, and gives out nitrous acid; this appears to be more hydrogenated than the former one.

Although the *coaly residuum* possesses some properties similar to those of pit-coal, the absence of sulphur and of sulphuric acid in this last, will not permit us to regard them as having a common origin. The analogous properties of these two substances appear to belong in general to all coaly substances which contain much hydrogen.

The astringent matter is soluble in water. Its solution appears green by reflection, and red by refraction: it precipitates gelatin; it is acid, and by distillation it affords sulphuretted hydrogen and sulphuric acid. It forms with

* I much regret not being able to examine the nature of this product. It would be interesting to know if it contains sulphuric acid and camphor. By distilling it with potass, I obtained an odorous crystallized sublimate, but it was in too small a quantity to enable me to determine exactly its nature.

barytes a compound soluble in water; it undergoes some alteration while the solution is evaporating; it appears that the sulphuric acid combined with it, re-acts upon the vegetable matter and blackens it. The sulphuric acid cannot be separated from the astringent matter without decomposing it.

In the analysis of vegetables, we are not to conclude that whenever any substance precipitates gelatin, that substance is tannin; for it is very probable, that there are several substances very different from each other, which possess that property. Lastly, since we find, 1st, that for the most part, those substances which form a precipitate with gelatin are acid; 2d, that frequently this animal matter cannot be precipitated from vegetable infusions, without the addition of an acid; and, 3d, that the greatest number of natural tannins redden turnsole; we may fairly presume that tannins are combinations of vegetable acids with substances of various natures.

LXXII. *Demonstration of the fundamental Property of the Lever. By* David Brewster, *LL.D. F.R.S. Edin.**

It is a singular fact in the history of science, that, after all the attempts of the most eminent modern mathematicians, to obtain a simple and satisfactory demonstration of the fundamental property of the lever, the solution of this problem given by Archimedes should still be considered as the most legitimate and elementary. Galileo, Huygens, De la Hire, Sir Isaac Newton, Maclaurin, Landen, and Hamilton have directed their attention to this important part of mechanics; but their demonstrations are in general either tedious and abstruse, or founded on assumptions too arbitrary to be recognised as a proper basis for mathematical reasoning. Even the demonstration given by Archimedes is not free from objections, and is applicable only to the lever, considered as a physical body. Galileo, though his demonstration is superior in point of simplicity to that of Archimedes, resorts to the inelegant contrivance, of suspending a solid prism from a mathematical lever, and of dividing the prism into two unequal parts, which act as the power and the weight. The demonstration given by Huygens assumes as an axiom, that a given weight removed from the fulcrum has a greater tendency to turn the lever

* From the Transactions of the Royal Society of Edinburgh.

roun[d]

round its centre of motion, and is, besides, applicable only to a commensurable proportion of the arms. The foundation of Sir Isaac Newton's demonstration is still more inadmissible. He assumes, that if a given power act in any direction upon a lever, and if lines be drawn from the fulcrum to the line of direction, the mechanical effort of the power will be the same when it is applied to the extremity of any of these lines; but it is obvious, that this axiom is as difficult to be proved as the property of the lever itself. M. De la Hire has given a demonstration which is remarkable for its want of elegance. He employs the *reductio ad absurdum*, and thus deduces the proposition from the case where the arms are commensurable. The demonstration given by Maclaurin has been highly praised; but if it does not involve a *petitio principii*, it has at least the radical defect, of extending only to a commensurable proportion of the arms. The solutions of Landen and Hamilton are peculiarly long and complicated, and resemble more the demonstration of some of the abstrusest points of mechanics, than of one of its simplest and most elementary truths.

In attempting to give a new demonstration of the fundamental property of the lever, which shall be at the same time simple and legitimate, we shall assume only one principle, which has been universally admitted as axiomatic, namely, *that equal and opposite forces, acting at the extremities of the equal arms of a lever, and at equal angles to these arms, will be in equilibrio.* With the aid of this axiom, the fundamental property of the lever may be established by the three following propositions.

In Prop. I. the property is deduced in a very simple manner, when the arms of the lever are commensurable.

In Prop. II., which is totally independent of the first, the demonstration is general, and extends to any proportion between the arms.

In Prop. III. the property is established, when the forces act in an oblique direction, and when the lever is either rectilineal, angular, or curvilineal. In the demonstrations which have generally been given of this last proposition, the oblique force has been resolved into two, one of which is directed to the fulcrum, while the other is perpendicular to that direction. It is then assumed, *that the force directed to the fulcrum has no tendency to disturb the equilibrium, even though it acts at the extremity of a bent arm*; and hence it is easy to demonstrate, that the remaining force i

proportional to the perpendicular drawn from the fulcrum to the line of direction in which the original force was applied. As the principle thus assumed, however, is totally inadmissible as an intuitive truth, we have attempted to demonstrate the proposition without its assistance.

PROP. I.

If one arm of a straight lever is any multiple of the other, a force acting at the extremity of the one will be in equilibrio with a force acting at the extremity of the other, when these forces are reciprocally proportional to the length of the arms to which they are applied.

Let AB (Plate X. fig. 1.) be a lever supported on the two fulcra F, f, so that $Af = fF = FB$. Then, if two equal weights C, D, of 1 pound each, be suspended from the extremities, A, B, they will be in equilibrio, since they act at the end of equal arms Af, BF; and each of the fulcra f, F, will support an equal part of the whole weight, or 1 pound. Let the fulcrum f be now removed, and let a weight E, of 1 pound, act upwards at the point f; the equilibrium will still continue; but the weight E, of 1 pound, acting upwards at f, is equivalent to a weight G of 1 pound, acting downwards at B. Remove, therefore, the weight E, and suspend the weight G from B; then, since the equilibrium is still preserved after these two substitutions, we have a weight C, of one pound, acting at the extremity of the arm AF, in equilibrio with the weights D and G, which together make two pounds, acting at the extremity of the arm FB. But FA is to FB as 2 is to 1; therefore an equilibrium takes place, when the weights are reciprocally proportional to the arms, in the particular case when the arms are as 2 to 1. By making Ff successively double, triple, &c. of FB, it may in like manner be shown, that, in these cases, the proposition holds true.

PROP. II.

If two forces, acting at the extremities of the two arms of a lever, and at equal angles to the arms, are in equilibrio, they will be reciprocally proportional to the lengths of the arms to which they are applied.

Let AB, CD (fig. 2.) be two levers in contact at AB, and forming one straight line ABCD. Bisect AB in f, and CD in φ, and from the extremities A, B, suspend equal weights m, m, and from the extremities C, D, equal weights n, n, so that $m : n = CD : AB$. If the two levers are now supported on the fulcra f, φ, they will both be in equilibrio; and

and will still form one straight line, the fulcrum f being loaded with a weight $= 2m$, and the fulcrum ϕ with a weight $= 2n$. Let us now suppose the extremities B, C, of the levers to adhere, and form one inflexible line AD; and let an inverted fulcrum F be placed at the point of junction. The equilibrium of the whole will evidently continue, and the fulcra f, ϕ, will be loaded as before. Remove the fulcra f, ϕ, and substitute in their place the weights $2m$, $2n$, acting upwards, and equal to the load which they respectively support: the equilibrium will still continue. Now, instead of the force m acting downwards at B, substitute an equal and opposite force m', acting upwards at A, and instead of the force n acting downwards at C, substitute an equal and opposite force n', acting upwards at D, and the equilibrium will still be preserved. But the two equal forces acting in opposite directions at the points A and D, destroy each other; therefore we have a force $2m$ acting at the extremity of the arm fF, in equilibrio with a force $2n$, acting at the extremity of the arm ϕF. But since, by the hypothesis, $m : n$ as CD : AB, and since fF is one-half of AB, and ϕF one-half of CD, we have $2m : 2n = \phi F : fF$, an analogy which expresses the fundamental property of the lever.

Lemma.

Two equal forces acting at the same point of the arm of a lever, and in directions which form equal angles with a perpendicular drawn through that point of the arm, will have equal tendencies to turn the lever round its centre of motion.

Let AB (fig. 3.) be a lever with equal arms AF, FB. Through the points A, B, draw AD, BE, perpendicular to AB; and AP, Ap, BW, Bw, forming equal angles with the lines AD, BE. Produce PA to M. Then, equal forces acting in the directions AP, Bw, will be in equilibrio. But a force M, equal to P, and acting in the direction AM, will counteract the force P, acting in the direction AB, or will have the same tendency to turn the lever round F; and the force W, acting in the direction BW, will have the same tendency to turn the lever round F as the force M; consequently the force W will have the same tendency to turn the lever round F as the force w.

Prop. III.

If a force acts in different directions at the same point in the arm of a lever, its tendency to turn the lever round its centre of motion will be proportional to the perpendiculars

let fall from that centre on the lines of direction in which the force is applied.

Let AB (fig. 4.) be the lever, and let the two equal forces BM, Bm, act upon it at the point B, in the direction of the lines BM, Bm. Draw BN, Bn, respectively equal to BM, Bm, and forming the same angles with the line PBω perpendicular to AB. To BM, Bm, BN, Bn, produced, draw the perpendiculars AY, Ay, AX, Ax. Now, the side AX = AY, and Ax = Ay, on account of the equality of the triangles, ABX, ABY; and if Ml, Mλ, be drawn perpendicular to Bω, the triangles ABY, BMl, will be similar, and also the triangles ABy, B$m\lambda$: hence we obtain

$$AB : AY = BM : Bl, \text{ and}$$
$$AB : Ay = BM : B\lambda$$

Therefore, *ex æquo*, $AY : Ay = Bl : B\lambda$.

Complete the parallelograms BMoN, B$m\omega n$, and Bl, Bλ will be respectively one-half of the diagonals Bo, Bω.

Now let two equal forces BM, BN, act in these directions upon the lever at B, their joint force will be represented by the diagonal Bo, and consequently one of the forces BM will be represented by $Bl = \frac{1}{2} Bo$. In the same manner, if the two equal forces Bm, Bn, act upon the lever at B, their joint force will be represented by Bω, and one of them, Bm, will be represented by $B\lambda = \frac{1}{2} B\omega$. Consequently the power of the two forces BM, Bm, to turn the lever round its centre of motion, is represented by Bl, Bλ, respectively; that is, the force BM is to the force Bm as Bl is to Bλ; that is, as AY is to Ay, the perpendiculars let fall upon the lines of their direction.

LXXIII. *Geological Observations, on unstratified Mountains, and on the Use and Abuse of Geological Theories.* By Mr. John Farey, *Senior.*

To Mr. Tilloch.

Sir, It gives me considerable pleasure, after so long a lapse, to observe Dr. William Richardson returning to the subject of geology, in which he has already achieved so much, and from whom we expect still greater things, and to see him among the contributors to the Philosophical and Geological Magazine; not that I mean to insinuate that the doctor's labours elsewhere on Fiorin Grass, have been unimportant or not highly useful; but I confess that I have not been

been able to refrain from joining, in the regret expressed of late by most of my friends, that it was not transferred, in great part at least, to those who could render less important aids to science.

It must be learnt with satisfaction by all well-wishers to geology, that a statistical survey is in hand, in which the stratification of Antrim will be fully treated of by this able observer, and I wish that in a future communication Dr. R. would inform us of the state of forwardness, and when this work may be expected to appear.

It is now near 20 years since Mr. William Smith discovered several large Hummocks resembling that of *Knocklaid* mentioned by Dr. Richardson, but on smaller scales probably, in the vicinity of Bath; and when I was receiving instructions from him ten years ago, he explained the several strata to me, on his coloured map, on a large scale, of the environs of Bath; and it will be learnt with surprise by Dr. Richardson and many others, that he then held (and does so yet for aught I know to the contrary) that these bold and solitary Hills were originally formed as they now stand, —a position which I could never give into, but combated from the first, with so much freedom, that we have since discussed this subject less than any others among his important discoveries, except that of Faults, on which latter subject, I learnt very little from Mr. S., nothing indeed, which could lead to a theory of them, or their important operations on the strata.

Although I fully agree with the conclusions of Dr. Richardson (p. 369), as to a great part of the Mountains and Hills being left behind, or carved out as it were, from more extended strata than we now see, as all those in Derbyshire will I think appear to be, from the facts I have stated in my Report on Derbyshire, lately published, vol. i. and in a paper some time ago presented to the Royal Society, yet, I am no less certain, that many other Mountains, and perhaps most of those having as Mr. Davy expresses it (page 392 of your last number) a crystalline texture and a stratification approaching to the perpendicular, and many others with somewhat different characters, may without impropriety be said *to be formed*, since their masses are the effects of nodular concretions, in which different substances were applied laterally and partially to or by the side of, or lapped round each other, in a manner perfectly distinct from the regular and parallel stratification of which Dr. Richardson speaks, and which have been the object of Mr. Smith's researches.

Imme-

Immediately on my return from viewing of Chernwood Forest, in Leicestershire, in August 1807, I stated my opinion, that this primitive tract, as some call it, not only formed one of these exceptions to regular stratification, but that it was a huge nodular concretion in the red earth or gypseous marl, underlaying the lias clay; and I have been more and more convinced by reading, correspondence and conversation ever since to the present time, that most, or perhaps every one of the tracts deemed primitive, or supposed hitherto, to be projecting points of the solid nucleus of the earth, in the British isles, are referable to similar concretions or anomalous masses in the upper, or in some of the inferior strata of these marls, of which I suspect that there are three at least, of very great thickness, interlaid with coal-measures and limestone rocks, also of great thickness. Whether the larger of the nodular concretions on the surface of the earth, such for instance as the lofty Alps, which I see strong reasons for referring to nodular masses in strata, that are underlaid by the chalk of London and Paris, were ever entirely covered by strata, that have been stript off them, or only partially on their skirts, that is, whether the world was ever as much larger, as to include *the summits* of these vast mountain chains or not, as Dr. Richardson hints, it may be very difficult to determine; but it will be easy I think to show, in the smaller nodular tracts, like that of Chernwood Forest and others in England, that the surrounding and covering strata have been denudated or swept away, and that in this sense they have been *left*, according to Dr. Richardson's concluding remark. I much wish that Dr. R. could be induced to extend his observations beyond the regularly stratified district to which, if I mistake not, they have been confined, to the nearest mountain or primitive tracts, and favour the world with his observations on the connection and distinctions between them; and for that purpose I have thrown out these hints for his consideration, and comparison with the phænomena: assuring him, that I am not less sensible of the importance of *facts* than himself, and hope to live to prove the same to the world: but as the philosophic end of all experimental or observed facts, is to obtain a *theory* or general knowledge of the principles which connect all those facts, my opinion is, that they ought to go on together; and that they may beneficially do so, I am satisfied fully, from my own experience in this particular pursuit, and in attending to the progress of chemical knowledge and others, of which I may state myself to be rather a spectator, than as taking any

any part in them. I may add, that theories seem perfectly harmless, when advanced by humble individuals like myself, and free from the evil justly complained of, that of their being retailed in books, lectures, &c. for ages together, because advanced by great men, without due inquiry into their validity, or any comparisons being made of the gross inconsistencies which may exist between such theories, and the most obvious of geological phænomena, and while many others of the most important kind, were either wholly unknown, or wilfully overlooked, by the framers of such theories (see your xxxiiid volume, page 313). Thus much I have been desirous of saying, on a subject, on which I daily witness greater mistakes than in any other, great numbers of persons having gone over from one extreme, that of implicitly receiving theories from great authority, to the not less mischievous one, of clamorously crying down all theories or attempts at generalization. I am determined however, not to be influenced by these circumstances, but always while viewing or contemplating the phænomena of nature, to give free scope to comparisons of them with existing theory, or to the imagination in search of others, which may seem to explain them better: and when afterwards, the facts and the reasonings or theory are faithfully stated together, it is impossible, I maintain, that any harm to science can come from them, while much good may result from the discussion which a new theory may excite.

I am, sir,

Your obedient servant,

Westminster, June 4, 1811. JOHN FAREY.

P. S.—The comparison which I have lately made with the List of Derbyshire *Hills* at page 162, of your present volume, and the proof of the Map of them engraved for my Report, have detected a few *errata* in the former, which I beg here to state for correction, viz. Page 165, line 4 from bottom, before *Gorsey* insert W.—Page 168, line 19, before *Wollaton* insert E.—P. 169, line 26, before *Cloud* insert W, and line 34, before *Mole* insert W.—P. 170, line 10, for (*Green*) read (*Blue*), and line 12, before *Ecton* insert W.—P. 171, line 4, for S read N, and line 7, before *Petycroft* insert S.—And p. 174, line 8, before *Lose* insert E.

LXXIV. *The*

LXXIV. *The Croonian Lecture, on some Physiological Researches respecting the Influence of the Brain on the Action of the Heart, and on the Generation of Animal Heat.* By Mr. B. C. Brodie, F.R.S.*

Having had the honour of being appointed, by the President of the Royal Society, to give the Croonian Lecture, I trust that the following facts and observations will be considered as tending sufficiently to promote the objects for which the lecture was instituted. They appear to throw some light on the mode in which the influence of the brain is necessary to the continuance of the action of the heart; and on the effect which the changes produced on the blood in respiration have on the heat of the animal body.

In making experiments on animals to ascertain how far the influence of the brain is necessary to the action of the heart, I found that when an animal was pithed by dividing the spinal marrow in the upper part of the neck, respiration was immediately destroyed, but the heart still continued to contract circulating dark-coloured blood, and that in some instances from ten to fifteen minutes elapsed before its action had entirely ceased. I further found that when the head was removed, the divided blood-vessels being secured by a ligature, the circulation still continued, apparently unaffected by the entire separation of the brain. These experiments confirmed the observations of Mr. Cruikshank† and M. Bichat‡, that the brain is not directly necessary to the action of the heart, and that when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of respiration. This led me to conclude, that, if respiration was produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain. The truth of this conclusion was ascertained by the following experiment.

Experiment 1.—I divided the spinal marrow of a rabbit in the space between the occiput and atlas, and, having made an opening into the trachea, fitted into it a tube of elastic gum, to which was connected a small pair of bellows, so constructed that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was kept up. I then secured the blood-vessels in the neck, and removed the

* From Philosophical Transactions for 1811, Part I.

† Philosophical Transactions 1795.

‡ *Récherches Physiologiques sur la Vie et la Mort.*

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the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract, apparently with as much strength and frequency as in a living animal. I examined the blood in the different sets of vessels, and found it dark-coloured in the venæ cavæ and pulmonary artery, and of the usual florid red colour in the pulmonary veins and aorta. At the end of twenty-five minutes from the time of the spinal marrow being divided, the action of the heart became fainter, and the experiment was put an end to.

With a view to promote the inquiry instituted by the Society for promoting the knowledge of animal chemistry respecting the influence of the nerves on the secretions*, I endeavoured to ascertain whether they continued after the influence of the brain was removed. In the commencement of the experiment I emptied the bladder of its contents by pressure; at the end of the experiment the bladder continued empty.

This experiment led me to conclude, that the action of the heart might be made to continue after the brain was removed, by means of artificial respiration, but that under these circumstances the secretion of urine did not take place. It appeared, however, desirable to repeat the experiment on a larger and less delicate animal, and that, in doing so, it would be right to ascertain whether, under these circumstances, the animal heat was kept up to the natural standard.

Experiment 2.—I repeated the experiment on a middle-sized dog. The temperature of the room was 63° of Fahrenheit's thermometer. By having previously secured the carotid and vertebral arteries, I was enabled to remove the head with little or no hæmorrhage. The artificial respirations were made about twenty-four times in a minute. The heart acted with regularity and strength.

At the end of 30 minutes from the time of the spinal marrow being divided, the heart was felt through the ribs contracting 76 times in a minute.

At 35 minutes the pulse had risen to 84 in a minute.

At one hour and 30 minutes the pulse had risen to 88 in a minute.

At the end of two hours it had fallen to 70, and at the end of two hours and a half to 35 in a minute, and the artificial respiration was no longer continued.

* Philosophical Transactions for 1809.

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By means of a small thermometer with an exposed bulb, I measured the animal heat at different periods.

At the end of an hour the thermometer in the rectum had fallen from 100° to 94°.

At the end of two hours a small opening being made in the parietes of the thorax, and the ball of the thermometer placed in contact with the heart, the mercury fell to 86°, and half an hour afterwards in the same situation it fell to 78°.

In the beginning of the experiment I made an opening into the abdomen, and having passed a ligature round each artery about two inches below the kidney, brought the edges of the wound in the abdomen together by means of sutures. At the end of the experiment no urine was collected in the ureters above the ligatures.

On examining the blood in the different vessels, it was found of a florid-red colour in the arteries, and of a dark colour in the veins, as under ordinary circumstances.

During the first hour and a half of the experiment there were constant and powerful contractions of the muscles of the trunk and extremities, so that the body of the animal was moved in a very remarkable manner, on the table on which it lay, and twice there was a copious evacuation of fæces.

Experiment 3.—The experiment was repeated on a rabbit. The temperature of the room was 60°. The respirations were made from 30 to 35 in a minute. The actions of the heart at first were strong and frequent: but at the end of one hour and forty minutes the pulse had fallen to 24 in a minute.

The blood in the arteries was seen of a florid red, and that in the veins of a dark colour.

A small opening was made in the abdominal muscles, through which the thermometer was introduced into the abdomen, and allowed to remain among the viscera.

At the end of an hour the heat in the abdomen had fallen from 100° to 89°. At the end of an hour and forty minutes in the same situation the heat had fallen to 85°, and when the bulb of the thermometer was placed in the thorax in contact with the lungs the mercury fell to 82°.

It has been a very generally received opinion that the heat of warm-blooded animals is dependent on the chemical changes produced on the blood by the air in respiration. In the two last experiments the animals cooled very rapidly, notwithstanding the blood appeared to undergo the usual

changes

changes in the lungs, and I was therefore induced to doubt whether the abovementioned opinion respecting the source of animal heat is correct. No positive conclusions however could be deduced from these experiments. If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard, or otherwise, must depend on the quantity of air inspired, and on the quantity of blood passing through the lungs in a given space of time: in other words, it must be in proportion to the fullness and frequency of the pulse, and the fullness and frequency of the inspirations. It therefore became necessary to pay particular attention to these circumstances.

Experiment 4.—The experiment was repeated on a dog of a small size, whose pulse was from 130 to 140 in a minute, and whose respirations, as far as I could judge, were performed from 30 to 35 times in a minute.

The temperature of the room was 63°. The heat in the rectum of the animal at the commencement of the experiment was 99°. The artificial inspirations were made to correspond as nearly as possible to the natural inspirations both in fullness and frequency.

At 20 minutes from the time of the dog being pithed, the heart acted 140 times in a minute with as much strength and regularity as before: the heat in the rectum had fallen to $96\frac{1}{2}$.

At 40 minutes the pulse was still 140 in a minute: the heat in the rectum $92\frac{1}{2}$.

At 55 minutes the pulse was 112, and the heat in the rectum 90°.

At one hour and ten minutes the pulse beat 90 in a minute, and the heat in the rectum was 88°.

At one hour and 25 minutes the pulse had sunk to 30, and the heat in the rectum was 85°. The bulb of the thermometer being placed in the bag of the pericardium, the mercury stood at 85°, but among the viscera of the abdomen it rose to $87\frac{1}{2}$.

During the experiment there were frequent and violent contractions of the voluntary muscles, and an hour after the experiment was begun, there was an evacuation of fæces.

Experiment 5.—The experiment was repeated on a rabbit, whose respirations, as far as I could judge, were from 30 to 40 in a minute, and whose pulse varied from 130 to 140 in a minute. The temperature of the room was 57°. The heat in the rectum, at the commencement of the experiment,

periment, was $101\frac{1}{2}$. The artificial respirations were made to resemble the natural respirations as much as possible, both in fullness and frequency.

At 15 minutes from the time of the spinal marrow being divided, the heat in the rectum had fallen to $98\frac{1}{2}^{\circ}$.

At the end of half an hour the heart was felt through the ribs, acting strongly 140 times in a minute.

At 45 minutes the pulse was still 140; the heat in the rectum was 94°.

At the end of an hour the pulse continued 140 in a minute; the heat in the rectum was 92°; among the viscera of the abdomen 91°; in the thorax, between the lungs and pericardium, 92°.

During the experiment, the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual.

The rabbit voided urine at the commencement of the experiment; at the end of the experiment no urine was found in the bladder.

Experiment 6.—I procured two rabbits of the same colour, but one of them was about one-fifth smaller than the other. I divided the spinal marrow of the larger rabbit between the occiput and atlas. Having secured the vessels in the neck, and removed the head, I kept up the circulation by means of artificial respiration as in the former experiments. The respirations were made as nearly as possible similar to natural respirations.

In 23 minutes after the spinal marrow was divided, the pulse was strong, and 130 in a minute: the ball of the thermometer being placed among the viscera of the abdomen, the mercury stood at 96°.

At 34 minutes the pulse was 120 in a minute; the heat in the abdomen was 95°.

At the end of an hour the pulse could not be felt; but on opening the thorax the heart was found acting, but slowly and feebly. The heat in the abdomen was 91°; and between the lobes of the right lung 88°.

During the experiment, the blood in the arteries and veins was seen to have its usual colour.

In this therefore, as in the preceding experiments, the heat of the animal sunk rapidly, notwithstanding the continuance of the respiration. In order to ascertain whether any heat at all was generated by this process, I made the following comparative experiment. The temperature of the room being the same, I killed the smaller rabbit by dividing

dividing the spinal marrow between the occiput and atlas. In consequence of the difference of size, *cæteris paribus*, the heat in this rabbit ought to diminish more rapidly than in the other; and I therefore examined its temperature at the end of 52 minutes, considering that this would be at least equivalent to examining that of the larger rabbit at the end of an hour. At 52 minutes from the time of the smaller rabbit being killed, the heat among the viscera of the abdomen was 92°, and between the lobes of the right lung it was 91°. From this experiment, therefore, it appeared not only that no heat was generated in the rabbit, in which the circulation was maintained by artificial respiration, but that it even cooled more rapidly than the dead rabbit.

At the suggestion of professor Davy, who took an interest in the inquiry, I repeated the foregoing experiment on two animals, taking pains to procure them more nearly of the same size and colour.

Experiment 7.—I procured two large full grown rabbits, of the same colour, and so nearly equal in size, that no difference could be detected by the eye.

The temperature of the room was 57°, and the heat in the rectum of each rabbit previous to the experiment was 100½.

I divided the spinal marrow in one of them, produced artificial respiration, and removed the head after having secured the vessels in the neck. The artificial respirations were made about 35 times in a minute.

During the first hour, the heart contracted 144 times in a minute.

At the end of an hour and a quarter the pulse had fallen to 136 in a minute, and it continued the same at the end of an hour and a half. At the end of an hour and forty minutes the pulse had fallen to 90 in a minute, and the artificial respiration was not continued after this period.

Half an hour after the spinal marrow was divided, the heat in the rectum had fallen to 97°.

At 45 minutes the heat was 95½.

At the end of an hour the heat in the rectum was 94°.

At an hour and a quarter it was 92°.

At an hour and a half it was 91°.

At an hour and forty minutes, the heat in the rectum was 90½, and in the thorax, within the bag of the pericardium, the heat was 87½.

The temperature of the room being the same, the second rabbit was killed by dividing the spinal marrow, and the temperature was examined at corresponding periods.

 Half

Half an hour after the rabbit was killed, the heat in the rectum was 99°.

At 45 minutes it had fallen to 98°.

At the end of an hour the heat in the rectum was 96½.

At an hour and a quarter it was 95°.

At an hour and a half it was 94°.

At an hour and forty minutes the heat in the rectum was 93°, and in the bag of the pericardium 90½.

The following table will show the comparative temperature of the two animals at corresponding periods.

Time.	Rabbits with artificial respiration.		Dead Rabbit.	
	Therm. in the Rectum.	Therm. in the Pericardium.	Therm. in the Rectum.	Therm. in the Pericardium.
Before the Experiment	100½		100½	
30 min.	97		99	
45 —	95½		98	
60 —	94		96¼	
75 —	92		95	
90 —	91		94	
100 —	90½	87½	93	90½

In this experiment, the thorax, even in the dead animal, cooled more rapidly than the abdomen. This is to be explained by the difference in the bulk of these two parts. The rabbit in which the circulation was maintained by artificial respiration, cooled more rapidly than the dead rabbit, but the difference was more perceptible in the thorax than in the rectum. This is what might be expected, if the production of animal heat does not depend on respiration, since the cold air by which the lungs were inflated, must necessarily have abstracted a certain quantity of heat, particularly as its influence was communicated to all parts of the body, in consequence of the continuance of respiration.

It was suggested that some animal heat might have been generated, though so small in quantity as not to counterbalance the cooling powers of the air thrown into the lungs. It is difficult, or impossible, to ascertain with perfect accuracy, what effect cold air thrown into the lungs would have on the temperature of an animal under the circumstances of the last experiment, independently of any chemical action on the blood; since, if no chemical changes were produced, the

the circulation could not be maintained; and if the circulation ceased, the cooling properties of the air must be more confined to the thorax, and not communicated in an equal degree to the more distant parts. The following experiment, however, was instituted, as likely to afford a nearer approximation to the truth, than any other that could be devised.

Experiment 8.—I procured two rabbits of the same size and colour: the temperature of the room was 64°. I killed one of them by dividing the spinal marrow, and immediately, having made an opening into the left side of the thorax, I tied a ligature round the base of the heart, so as to stop the circulation. The wound in the skin was closed by a suture. An opening was then made into the trachea; and the apparatus for artificial respiration being fitted into it, the lungs were inflated, and then allowed to collapse as in the former experiment, about 36 times in a minute. This was continued for an hour and a half, and the temperature was examined at different periods. The temperature of the room being the same, I killed the second rabbit in the same manner, and measured the temperature at corresponding periods. The comparative temperature of the two dead animals, under these circumstances, will be seen in the following table.

Time.	Dead Rabbit whose lungs were inflated.		Dead Rabbit whose lungs were not inflated.	
	Therm. in the Rectum.	Therm. in the Thorax.	Therm. in the Rectum.	Therm. in the Thorax.
Before the Experiment	100		100	
30 min.	97		98	
45 —	$95\frac{1}{2}$		96	
60 —	94		$94\frac{1}{2}$	
75 —	$92\frac{1}{2}$		93	
90 —	91	86	$90\frac{1}{2}$	$88\frac{1}{2}$

In this last experiment, as may be seen from the above table, the difference in the temperature of the two rabbits, at the end of an hour and a half, in the rectum, was half a degree, and in the thorax two degrees and a half; whereas, in the preceding experiment, at the end of an hour and forty minutes, the difference in the rectum was $2\frac{1}{2}$ degrees, and in the thorax 3 degrees. It appears, therefore, that the rabbit in which the circulation was maintained by artifical re-

spiration, cooled more rapidly on the whole than the rabbit whose lungs were inflated in the same manner after the circulation had ceased. This is what might be expected if no heat was produced by the chemical action of the air on the blood; since in the last case the cold air was always applied to the same surface, but in the former it was applied always to fresh portions of blood, by which its cooling powers were communicated to the more distant parts of the body.

In the course of the experiments which I have related, I was much indebted to several members of the Society for promoting the Knowledge of Animal Chemistry, for many important suggestions which have assisted me in prosecuting the inquiry. Mr. Home, at my request, was present at the seventh experiment. Dr. E. N. Bancroft was present at, and assisted me in, the second experiment; and Mr. Wm. Brande lent me his assistance in the greater part of those which were made. I have been further assisted in making the experiments by Mr. Broughton, surgeon of the Dorsetshire regiment of militia, and Mr. Richard Rawlins and Mr. Robert Gatcombe, students in surgery.

I have selected the above from a great number of similar experiments, which it would be needless to detail. It is sufficient to state, that the general results were always the same; and that whether the pulse was frequent or slow, full or small, or whether the respirations were frequent or otherwise, there was no perceptible difference in the cooling of the animal.

From the whole we may deduce the following conclusions:

1. The influence of the brain is not directly necessary to the action of the heart.

2. When the brain is injured or removed, the action of the heart ceases, only because respiration is under its influence; and if under these circumstances respiration is artificially produced, the circulation will still continue.

3. When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

4. When the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate, but to diminish animal heat.

LXXV. *Fatal*

LXXV. *Fatal Case of Umbilical Hernia.* *By* JOHN TAUNTON, *Esq. Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society, Lecturer on Anatomy, Surgery, Physiology, &c.*

17*th June* 1811.—7*h*. P. M.—DOCTOR HANCOCK and myself visited Mrs. Tranter: she was aged about 33 years, had enjoyed good health generally, and been the mother of several children, (seven of whom are living,) and a woman according to report of very industrious habits. She had had an umbilical hernia for several years, which did not appear to have been reduced for some time past. On Friday last the 14th instant it became inflamed and painful: this was succeeded with the usual symptoms of strangulated hernia. A medical man was called in on Saturday morning, who bled her, applied fomentations and poultices, and directed that leeches should be applied to the part; he also administered a great number of clysters, six or seven of them contained each an infusion of a quarter of an ounce of tobacco. The unfavourable symptoms continued to increase; and the patient not having the means of paying for more medicines, her husband obtained a letter for the Finsbury Dispensary this day. The patient was then told that the only thing to be done was to cut the body open, and return the gut; an operation, from the manner in which it was named, that excited great horror in all persons present. This account was given to us by the poor woman and females about her, and confirmed by her husband.

The sickness and vomiting distressed her very much: the hernia, which was not large, was painful and tense: the integuments covering it had a livid unfavourable appearance: the operation appeared to be the only chance of affording relief: but from the unfavourable impression, or rather horror, on their minds, they solicited that some means should be tried first, at least for an hour or two. Four pills were then ordered, each of which contained one grain of opium, two grains of calomel, and one drop of the oil of mint: one of these pills was directed to be taken every half-hour.

Dr. H. and myself visited the patient again at 9 P. M. and took the instruments with a view of performing the operation. She had then taken three of the pills, and had not had any return of the pain, sickness, hiccough, or vomiting, from the time she took the first pill; the hernia was become much softer, and less painful on pressure.

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She appeared in every respect much relieved, and expressed herself confident of getting well without the operation; a gentle perspiration was diffused over the body, the pulse was stronger and less frequent than on our first visit at seven P. M. (i. e. two hours ago), the tongue was moist, and not much furred.

18th, six A. M. She had slept for some hours during the night, expressed herself greatly relieved, had taken some toast and water, had not had any return of the pain, sickness, hiccough, or vomiting, was in a gentle perspiration, the tongue moist, pulse about 80 beats in a minute and moderately full: she had had the common clyster given during the night, but this returned without any stool: the hernia was soft; but the integuments retained that livid appearance which made an unfavourable impression on my mind as to the result of the case.

I visited her again at one A. M. The symptoms were the same as this morning at six o'clock, excepting that there was more anxiety in the countenance, and that she seemed to be in a restless state rather than in pain: the operation was recommended again, and assented to on her part.

At one P. M. the operation was performed in the presence of Dr. Hancock and three other gentlemen. On opening the hernial sac, which was very distinct, some feculent matter escaped; the omentum, which formed the bulk of the tumour, was not much inflamed: on separating the omentum, a very small convolution of one of the small intestines was exposed, closely embraced by the stricture: the intestine was in a complete state of gangrene, and had given way, though the opening was not visible: the stricture was dilated in the direction of the linea alba downwards: the intestine readily receded; but the opening into it, through which the fæces and air passed, remained opposite the wound: great part of the omentum was removed, and its adhesions left at the mouth of the sac, with a view of its assisting in supporting and retaining the intestine to that part; one ligature only was required on a very small artery of the omentum. She appeared as well after the operation as is usual, and hoped that there was not then any danger. In about an hour she became very restless, and died between two and three hours after the operation.

It appears to me, that the effects which took place immediately on taking the pills were produced by the malady terminating at that time in gangrene, rather than from the medicine. Is it not highly probable, therefore, that the gangrene and even the death of the patient were greatly accelerated

celerated by the powerful narcotic properties of the tobacco employed in this instance in a most unusual and unwarrantable manner?

This is another of the many cases which prove the necessity of having recourse to an early operation, the extreme fallacy and even danger in trusting to medicine, and the inhumanity in representing the operation in such terms as to prejudice an unfortunate sufferer against its early performance.

The many invaluable charitable institutions in this metropolis, established for the express purpose of affording relief to the afflicted poor; the easy and daily access to these charities, particularly dispensaries, and societies for the relief of the ruptured poor, render it, in my opinion, highly criminal in any practitioner who does not choose to perform the necessary operations himself, to retain the patients under his care for the sake of paltry gain, which is often acquired at the expense of the life of a parent of a large family, who, if industrious and prudent, though poor, is one of the most valuable members of the community.

John Taunton.

Greville-street, Hatton-garden, 19th June, 1811.

LXXVI.—*On saving the Lives of Mariners.*

It is surprising that a subject of so much importance should have received comparatively but little consideration till within these few years; but it is consolatory to observe that, the attention of the public having been at length called to it, benevolent and intelligent men have occupied their minds, and applied their inventive powers, to the contriving of remedies for the dangers attending shipwreck, with a degree of energy and success that could hardly have been hoped for.

In the year 1792, the Society for the Encouragement of Arts, Manufactures, and Commerce, gave a bounty of fifty guineas to Mr. John Bell, then a serjeant, afterwards a lieutenant in the royal artillery, for his invention of throwing a rope on shore by means of a mortar from a vessel in danger of shipwreck *; and in the year 1807, the same Society published some further particulars, with a plate of the apparatus †. Having already laid before our readers an account of this invention‡, it is only necessary to observe

* See the Society's Transactions, vol. x.

† Ibid. vol. xxv.

‡ Phil. Mag. vol. xxxii. p. 294.

respecting it, that its uses have since been extended, and the invention itself much improved, by Capt. Manby, as appears by the following copy of a

" *Report from the Committee of Field Officers of Artillery, containing an Account of the Experiments made at Woolwich, on the 18th and 20th May last, on Captain Manby's Invention for saving the Lives of Shipwrecked Mariners.* (Printed by Order of the House of Commons.)

" SIR, Royal Arsenal, Woolwich, 22d May, 1811.

" In obedience to the honourable board's commands, transmitted in your letters of the 29th ultimo and the 3d instant, I assembled the committee of colonels and field officers of royal artillery, named below*, on the 18th and 20th instant, to take into consideration, and to give their opinion on, Captain Manby's discovery of an instantaneous manner of discharging pieces of ordnance for the relief of shipwrecked persons, without the application of fire; and further, to investigate the several subjects stated in Captain Manby's letter to the honourable board, of the 2d instant, wherein he requests the report to extend ' to the whole of his various productions, as they are now considered by him complete, and to the fullest of his wishes.'

" The committee having communicated with Captain Manby on the subject of their meeting, he submitted to them his arrangement of the proposed experiments; after which they adjourned to the ground for mortar-practice, in the barrack-field, where he exhibited them in the following order:

" 1st—' A person completely equipped with every necessary apparatus to effect a communication with a vessel driven on a lee shore.'

" A man mounted on horseback was exhibited, accoutred with a deal frame, containing 200 yards of log line, ready coiled for service, which was slung as a knapsack, with a brass howitzer of a 3-pounder bore on its carriage, and two rounds of ammunition, the whole weighing 62 pounds, strapped on the fore part of the saddle. The person thus equipped, is supposed to be enabled to travel with expedition to the aid of ships in danger of being wrecked, on parts of the coast intermediate to the mortar stations: and with this small apparatus the log line is to be projected

* Present, Lieut. Gen. Lloyd, Major Gen Ramsay, Col. Borthwick, Lieut. Col. Rion, Lieut. Col. Spicer, Lieut. Col. Colebrooke, Lieut. Col. Beever, Major Gold, Major Buckner.

over the vessel in distress, from which a rope should be attached to it to haul the crew on shore.

"Captain Manby caused the howitzer to be dismounted from the horse, and in a very few minutes fired it, when the shot was thrown, with the line attached, to the distance of 143 yards, with $2\frac{1}{2}$ ounces of powder. In this experiment, Captain Manby used a kind of pear shot $1\frac{1}{2}$ diameter in length, and weighing 4lbs. 12oz. 12dr. by which additional weight the shot's momentum and power over the line is considerably augmented, though the recoil is increased in proportion; which does not appear to be a consideration of moment, when compared with the importance of communicating with the distressed vessel.

"At a subsequent trial, the horseman, fully equipped, travelled $1\frac{1}{2}$ mile, the howitzer was dismounted, and the line projected 153 yards in six minutes.

> "2d—'Insuring the means of firing ordnance, and thereby affording relief, by a rope being projected, when the severities of storm prevent the possibility of a match being kept alight for that purpose; as on the success of this service every thing depends.'

"Captain Manby exhibited a mode of firing ordnance by the chemical agency of two substances, which ignite when coming in contact with each other. The effect was certain and instantaneous, and particularly well adapted to the services he proposes, which frequently happens during severities of weather, when it is most difficult to keep matches alight, or to make the fire of a lock take effect; and in situations where, from the impossibility of renewing a light, the delay might prove fatal.

> "3d—Captain Manby showed 'the construction and mode of laying and firing a piece of ordnance from a boat, when the sea is continually breaking over it, to communicate with a vessel that has grounded on a bar, in running for a harbour in a storm, to approach which, from the broken water, it has been found impossible to get to her without such aid.'

"On this occasion, a 12-pounder howitzer was fired with six ounces of powder, which projected a shot and a deep sea-line 74 yards. The explosion shattered a wooden cover to pieces, which was placed over the howitzer to preserve it from the waves, and struck several by-standers with violence, proving its application at the moment of firing to be dangerous, and particularly in a boat where the men must be very close to the piece. The committee think the

the cover may be of great service, provided the necessary precaution is adopted of removing it at the time of firing.

"4th—The next experiment consisted of 'the readiest method of giving assistance, by the rope being laid and conveyed to the spot in a basket; and another certain method of firing the piece.'

"The committee are of opinion, the application of the basket must be attended with considerable advantage, from its portability, and saving much time that would be required for coiling the rope on the ground.

"5th—A rope ladder was exhibited, 'intended to be projected or conveyed to a crew wrecked under a cluff or inaccessible cliff.'

"This ladder consists of a single rope, with loops spliced to it at convenient distances, for the support of the feet and hands when climbing. The ladder of this construction was attached to a 24-pounder shot, and fired from a $5\frac{1}{2}$ inch mortar at 19° elevation with 12 ounces of powder. Though this experiment failed, by the ladder breaking, which Captain Manby attributes to the hardness of the rope, the committee see no reason why it may not be of the greatest utility when formed of proper materials: and they have to add, that on the following day Captain Manby repeated the same experiment with perfect success, projecting the ladder to the distance of 194 yards with the before-mentioned charge and elevation.

"6th—In order to show 'a method of affording certain relief to vessels stranded in the darkest night, with an improved mode of rendering the life-rope more distinguishable;'

"Light balls were thrown into the air from a mortar, at 80° elevation, with three ounces and two ounces of powder; and the $5\frac{1}{2}$ inch mortar, charged with eight ounces of powder, projected a deep-sea-line, attached to a shell with four fuzes, to the distance of 159 yards. Though this trial was not attended with the desired effect, there can be no difficulty in rendering light balls efficacious in Captain Manby's service, as has already been stated in my Report of the 3d May, 1809, when they succeeded perfectly; and at which time the committee also expressed their entire approbation of his method of illuminating the life-rope, as above described.

"7th—'The distance a deep-sea-line can be projected from the shortest constructed 8-inch mortar, as a deep-sea-line is of sufficient strength to send a hawser

to

to a vessel stranded on a very flat shore, which is consequently a considerable distance from the land.'

"With this view Captain Manby charged an 8-inch mortar with two pounds of powder, and with an elevation of 23° degrees projected a 68-pound shot, with the deep-sea-line, to the distance of 439 yards. The committee consider this application of the 8-inch mortar to promise great utility in the situations Captain Manby has described.

"8th—'To illustrate by experiment the method and distance an 8-inch barbed shot can be projected, for the purpose (when it is impossible without such aid) to haul a boat from a beach over a high raging surf, to go to ships in distress at a distance from the land, with a patent Sunderland 2-inch rope of uncommon strength, and which has actually saved, this winter, 29 persons.'

"Captain Manby had previously placed two anchors and buoys, united by a hawser, at two cables length distance from the mortar, the explosion of which, with two pounds of powder, broke the patent rope, and caused the experiment to fail in the first instance. Captain Manby afterwards repeated the trial with success, projecting the shot and rope 336 yards.

"The committee therefore consider this last proposition as being practicable, as far as the projection of the rope is concerned; the want of success at the first trial appearing to have arisen from accident in the mismanagement of the rope, to which casualties such experiments must always be subject.

"After the most careful attention to the experiments exhibited by Captain Manby, and the fullest consideration of all the improvements which he has made, the committee are of opinion they cannot too strongly recommend an invention, the partial application of which has been attended with such beneficial effects.

"It is also the wish of the committee to render their full tribute of praise to Captain Manby, for his ingenuity in so much improving and bringing into practical use this invention, to the perfecting of which he has so zealously and skilfully devoted himself.

"But the committee at the same time feel that they should not entirely discharge their duty, were they to omit observing, that the committee of the honourable House of Commons do not seem to have been informed of all the means proposed by the late Lieutenant Bell, of the royal artillery, for the attainment of the same laudable object; it being stated in that honourable committee's Report, that Mr.

Mr. Bell's invention 'is totally inapplicable in cases of vessels being stranded,' and that Captain Manby's invention is new.

"In justice therefore to the memory of Lieutenant Bell, and to his surviving family, and with respectful deference due to the judgement of that honourable committee, the concluding of the seven observations inserted in one of the papers of Lieutenant Bell's account to the Society for the Encouragement of Arts, Manufactures, and Commerce, is subjoined in his own words as published in that Society's Transactions, and in the Repertory of Arts for 1808, page 318; by which observations it appears that Lieutenant Bell then proposed what Captain Manby has since so ably and so successfully carried into effect.

"There is every reason to conclude that this contrivance would be very useful at all ports of difficult access both at home and abroad, where ships are liable to strike ground before they enter the harbour; as Shields Bar, and other similar situations; when a line might be thrown over the ship, which might probably be the means of saving both lives and property: and moreover, if a ship was driven on shore near such a place, the apparatus might easily be removed to afford assistance; and the whole performance is so exceedingly simple, that any person once seeing it done, would not want any further instruction. I have the honour to be, sir, your obedient humble servant,

(Signed) "VAUG. LLOYD, Col. Com. Lt. Gen."

"R. H. Crew, Esq. &c. &c. &c.

In consequence of the preceding Report, a motion was made by Mr. Wilberforce, on the 14th June, and carried, for an address to the Prince Regent, praying that he would be graciously pleased to order that Captain Manby's invention should be stationed on different parts of the coast, &c. and assuring him that the House would make good the expense.

In 1807, the Society for the Encouragement of Arts, &c. also rewarded a Mr. Daniel, for an apparatus to secure persons from sinking in water, or to act as a life-preserver when shipwrecked. This contrivance consists of a bag made of water-proof leather, in the shape of a broad girdle, encircling the person's body under his arm-pits, kept in its place by two straps over the shoulder and one passing between the legs. It is also furnished with a tube to blow it up by, and a cock to retain the inclosed air. This contrivance will certainly prevent a man from sinking, and even enable him to carry some weight along with him; but it

it is exceedingly cumbersome, nor could it be considered as original, something similar having been described in the German *Theatrum Machinarum.*

About the same time that the last-mentioned invention was brought before the public, a similar contrivance, but in a more objectionable shape, was also announced, and, if we rightly recollect, under the sanction of letters patent. It was in fact the same kind of girdle, but made of tinned iron in place of leather. It had no single requisite but that of buoyancy.

The merits of Mr. Greathead's life-boat have already been noticed in our pages*. It has been the means of saving many valuable lives to the community. In the year 1802 Parliament voted £1200 to Mr. Greathead as a reward for his invention. It appears by a petition from him to the House of Commons presented during the last session, and from the Report of the committee to whom the petition was referred, that of the above sum there was consumed by the solicitor's bill, including fees to both houses of parliament, £115 8s.; that the charges paid at the Treasury amounted to £67 9s. 6d.; and that Mr. G. further expended in the course of the necessary attendance of himself and witnesses before parliament the sum of £171 2s. 6d. In consideration of these circumstances, and of the general utility of the invention, the committee recommended to the house to grant, as a reasonable addition to his former reward, the sum of £650.

During the last session a petition from Mr. Henry Mallison, respecting an invention for preserving the lives of seamen, was referred to a committee. It consists in applying about three pounds weight of cork to a man's body by convenient means, and in such a manner that the person receives little or no impediment from it in the use of his limbs. It appeared, by the evidence laid before the committee, to be greatly superior to the common cork jacket. Our limits do not permit us to give the minutes of evidence; but the following Report is decisive of the merits of this invention.

"*Report from the Committee on the Petition of William Henry Mallison.*

"The committee, having examined the witnesses produced before them by the petitioner Mr. Mallison, together with his invention itself, to save persons from drowning, denominated by him 'The Seaman's Friend;' and having witnessed some actual experiments made in the river

* Philosophical Magazine, vol. xv. p. 331.

Thames

Thames by persons who had on the 'The Seaman's Friend,' both in swimming and rowing; have unanimously agreed upon the following Resolutions :

"1. Resolved,—That it is the opinion of this committee, That the application of cork, after the mode invented by Mr. Mallison, is effectual for the preservation of persons in the water; and it appears, from the evidence taken, that experiments have been made, as well by persons who could swim, as by those who could not, in the open sea and in rough water, and by one person in particular, a good swimmer, in a situation of uncommon peril, all of which have been quite successful.

"2. Resolved,—That it is the opinion of this committee, That the application of the invention to the crews of boats going off from ships to shore, or returning in stormy weather, would be exceedingly useful; and on all dangerous services of the same nature; especially as the use of the invention cannot materially impede the action of the limbs either in rowing, walking, or making any necessary exertion on the beach; and the committee have no doubt, that in many dreadful disasters which have happened, such as fire, or foundering of ships at sea, when in company with other vessels, (as in the instances of the Prince George, Admiral Broderick's ship, in the seven years war, and the Queen Charlotte, not many years since, in the Mediterranean,) if a quantity of 'The Seaman's Friend' invented by Mr. Mallison had been on board, many valuable lives would have been saved.

"3. Resolved,—That it is the opinion of this committee, That the invention of Mr. Mallison is well deserving of public attention."

We cannot close the present article without noticing the zealous labours of another individual in the cause of humanity; we mean Mr. Thomas Cleghorn, the author of a little volume entitled "*The Hydro-Aëronaut, or Navigator's Life-buoy,* being an easy and effectual Method of preventing the Loss of Lives by Drowning in *Cases of Shipwreck.*" The principal intention of this work is to show, that "without any particular precaution, every ship has almost always within itself effectual means to prevent the loss of any of its crew in cases of shipwreck, and others of a similar nature; and that man, naturally the most helpless of animals in water, may easily acquire buoyancy sufficient to support him individually in that element." The means he recommends are so simple, and at the same time so effectual, that it excites surprise to think they should for so great a length of time have been overlooked. Two-thirds

of

of a pint of confined air for every stone (14 pounds) a man weighs in air, will support him in fresh water with his head all above the surface; but allow a pint for each stone. In sea water half the above quantity will produce the same effect. A buoyancy of eight pints or one gallon of air will support a man in sea water, and at this rate a beer hogshead will sustain 54 men with their heads out of the water. Every ship contains *water-tight* (in this case *air-tight*) barrels of different kinds, more than sufficient to carry her crew were they ten times the number, and the only question is how to take advantage of the circumstance. Mr. Cleghorn has pointed out many methods, for which we must refer to his work; but we cannot pass over some of the most obvious. A single cask is apt to turn in the water; two are therefore preferable, for when lashed together they cannot upset. To three sides of this float, as many loops of rope may be attached by the lashing, or nails, as men can lay hold of. While they keep their hold they cannot sink, and in most cases of shipwreck they would be carried to the shore. If one of the boats be made a life-boat by lashing a hogshead or two to the boat, a rope from this boat might drag the cask-floats and the men attached to them. If two hogsheads be lashed to the two ends of a yard, or any long piece of timber, and two ropes pass from hogshead to hogshead at the side not occupied by the timber, a kind of life-boat may be formed, capable of sustaining a number of men,—the yard or other piece of timber forming the keel, and the two ropes the gunwale of the boat. But Mr. C. does not confine himself merely to such contrivances, though certainly very effectual: a tin canister weighing about one pound, and capable of containing $4\frac{1}{2}$ pints (*i.e.* a common pound and half tea canister) is able to sustain a man with his head above the surface of the water. Two empty common quart bottles well corked, put into a man's pocket, and the pockets brought up to his breast, are sufficient to float him.

The obvious conclusion from all this is, that a number of buoys of cork, or flat tin canisters (both cheap articles), should be provided on board every ship; and that the men ought to be trained a little to the lashing of casks, yards, &c. with a view to this very object, that on any emergency they might proceed without embarrassment or confusion to prepare means for their own safety.

Mr. Wilson's Life-boat has likewise been described in a previous volume*; as has also Mr. Knight Spencer's Marine Spencer †.

* Vol. xxxi. p. 259. † Vol. xvi. p. 172 and 272.

LXXVII. *Pro-*

LXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 30.—The president in the chair, the reading of Mr. Travers's paper was concluded. It consisted of a summary of his experiments on wounds made in the cavity of the body, as it is somewhat improperly denominated. By these it appeared that the part called a cavity is always so completely full, that no extravasation can take place in consequence of a horizontal or longitudinal puncture of the intestines, as in one case the lips of the wound are closed by pressure and cohesion, and in the other by inflammation. The Society then adjourned on account of the Whitsun holidays to

The 13th of June; when a curious account was read of a fœtus having been taken from the body of a woman, where it had remained 52 years. The narrative was written by Dr. Chester, who examined the body after death. The woman was a native of Gloucester, had been taken in labour as usual, but owing to the unskilfulness of the midwife was not delivered. A surgeon was sent for; but when he arrived the pains of travail had subsided, and in seven or eight days the woman's health recovered without delivery, and she lived to the age of 80, when she died of paralysis. Dr. C. learning the history of the case, opened the body, and found an ossified globe which contained the perfect child, the arms and legs of which were somewhat compressed by this osseous cyst, and in some parts partial absorption had taken place. The fœtus was livid but not putrid, and no mortification or corruption appeared: the bony shell in which it was enveloped was of considerable thickness and durability.

A paper on the alcohol of wine, &c. by Mr. Brande was read. The object of this chemist was either to confirm or refute the opinion of Fabroni, that alcohol is a product of distillation, and not an essential part of the vegetable liquor. He began by trying Port wine with subcarbonate of potash, which gave no indications of alcohol till a considerable quantity of spirits was added, when the presence of alcohol was manifested. Mr. B. concluded, that if alcohol were really a product, its quantity must depend on the heat applied in distillation; and with this view he distilled wine for several days at 180 degrees, which yielded precisely the same quantity of alcohol as that distilled more rapidly at 200°. In conclusion, he gave a table of the quantity

quantity of alcohol contained in various wines and malt liquors: the highest was that of Marcella wine, which contains 26 per cent. of alcohol; red Champagne 20, Port from 20 to 24, Madeira 19, claret 15, cider and perry 12, ale 9, brown stout 8, porter 6.

June 20.—A long paper by Dr. Herschel on *Nebulæ* was read: in this the Doctor retracted some of his former opinions respecting *nebulæ*, that they might be considered as clusters of stars. At present he concludes them to be peculiar condensed matter, supposes that they may constitute or become comets, regrets our inability to form any just notions of their mode of existence, presumes that they are much more numerous than we have hitherto been taught to believe, examines the general appearance of nebulosity in the heavens before space-penetrating glasses of various foci, and refers generally to figures of nebulæ given in different publications by himself and others, and particularly in the *Connoissance des Tems* for 1784.

ROYAL INSTITUTION.

Mr. Davy's Lectures on Geology.—No. IV.

Besides the natural fissures of rocks, there are deep and extensive chasms of frequent occurrence, the corresponding sides and angles of which intimate their formation to have taken place after the consolidation of the rocks in which they are found. These chasms when filled up constitute veins.

Veins are the repositories of a great variety of substances, particularly of crystallized minerals and metallic ores. Their contents are in some measure connected with the nature of the rocks they traverse. In the primary class of rocks, veins are rarely metalliferous—they generally consist of secondary rocks. In the secondary class, veins are found most abundant in metals. Tin occurs in veins in secondary granite; and copper, tin, lead, iron, silver, quicksilver, manganese, &c. in argillaceous and siliceous schist, and in the traps and grey wackes. Lead is the most plentiful ore in the secondary limestones. Veins containing metallic ores seldom occur in shales, sandstones, or basalts.

Different metallic ores are pretty regularly associated in veins with different minerals; and the latter, when they appear, are said to be indications of the former: thus an ochrey powder, called gossan in Cornwall, denotes the proximity of copper, and a green earth, a species of chlorite, the proximity of tin. Other circumstances, such as

water

water impregnated with metallic salts, the sterility of certain spots of land, the frequent appearance of light in particular places—an electrical phænomenon, probably, owing to the great conducting power of metallic ores, &c. guide the miner in his search. Metallic veins are sometimes discovered by means of the fragments separated from their surface: the tracing of these scattered fragments to their origin is in Cornwall called shodeing. The divining rod, which is a forked hazel twig, was formerly in great repute for discovering metallic veins and springs of water. The rod itself can only be influenced by the hand holding it; and as metallic veins and moist strata are superior conductors of electricity, a slight electrical effect consequently may be produced on persons possessed of great sensibility, in their neighbourhood. But the effect is so vague, that it affords no useful or certain indication. And it is a vulgar mistake, that water is confined to any particular part of a stratum; it is usually diffused through the sand, gravel, and clay composing it, and will collect wherever pits are made for its reception.

The direction of veins, though their dispositions and ramifications are very irregular, is generally uniform. The direction of metallic veins is, with little deviation, from east to west. Veins the course of which is from north to south are seldom metalliferous. These veins pass uninterrupted through the preceding, sometimes merely cutting their line of direction, at others removing laterally one part of the divided vein to some distance from the other. The derangement thus produced is in Cornwall called the heave of the lode or vein.—Cross veins of basalt are curious, on account of the horizontal position of the pillars into which they are commonly split.

Basaltic veins are of the latest formations: consequently, as they intersect and derange metallic veins, it necessarily follows that at present there is no production or renovation of ores, and that the now vulgar idea that the exhausted parts of mines are replenished, is erroneous. Mr. Davy noticed the hypothesis of Becher, Henkel, and Lehman, respecting metallic veins, founded on this notion.

Metallic veins, like the secondary rocks, are the production of an ancient and obscure period, when the ocean overspread the surface of the present land. And before the mysterious subject of their formation can be understood, there must be a great extension of chemical discovery, as well as a collection of geological facts.

Lecture V.—In this lecture Mr. Davy considered the causes

causes that produce the decomposition and decay of the surfaces of rocks, and the effects resulting in consequence in the œconomy of nature.

The changes of temperature, of the surface, of the action of electricity, of air, and of water, constantly tending to the disintegration of the superficial parts of rocks and mountains, and their operation, are different in different cases.

The expansion of water in the pores or fissures of rocks by heat or congelation, is a physical cause of the separation of their parts. The solvent power of moisture exerted upon alkaline or calcareous matter in rocks, is another cause of their decomposition. The action of carbonic acid upon the lime or magnesia, and of oxygen upon the iron they contain, produces an analogous result. Electricity, which is shown by experiments with the Voltaic apparatus to be a most powerful agent of decomposition, seems to assist in all these changes; and electrical powers are almost constantly exhibited in the atmosphere, sometimes occasioning the magnificent and awful effects of thunder-storms; at other times, by slow operation, tending to produce new arrangements of the elements of matter.

The primary rocks, as was noticed in a former lecture, are less decomposable than the secondary; and amongst the primary rocks, the serpentines, micaceous schists, marbles, and porphyries, are more decomposable than the granites: amongst the secondary rocks, shale, basalt, and soft sand-stone, are usually very subject to decay.

The granites and porphyries, by their decomposition, afford sand and porcelain clay; the serpentines and schists, usually a brown or yellow earth; the sandstones, sand; limestones, marles, shales, a black loam; and basalts, usually a red ochreous soil.

The end of the decomposition of rocks seems to be the production of a bed for vegetation. As soon as the rock begins to be softened, the seeds of lichens, which are constantly floating in the air, make it their resting-place. Their generations occupy it till a finely-divided earth is formed, which becomes capable of supporting mosses and heath: acted upon by light and heat, these plants drink in the dew, and convert constituent parts of the air into nourishment. Their death and decay afford food for a more perfect species of vegetable; and at length a mould is formed, in which even the trees of the forest can fix their roots, and which is capable of rewarding the labours of the cultivator.

The decomposition of rocks tends to the renovation of

soils,

soils, as well as to their formation. Finely-divided matter is carried by rivers from the higher districts to the low countries, and alluvial lands are usually extremely fertile. The quantity of habitable surface is constantly increased by these operations; precipitous cliffs are gradually made gentle slopes; lakes are filled up; and islands are formed at the mouths of great rivers.

In these series of changes, connected with the beauty and fertility of the surface of the globe, small quantities of solid matter are carried into the sea; but this seems fully compensated for by the effects of vegetation in absorbing matter from the atmosphere, by the production of coral rocks and islands in the ocean, and by the operation of volcanic fires.

By wise and beautiful laws, the equilibrium of things is constant. Life is preserved by operations which appear destructive; order and harmony arise from what at first view seems derangement and confusion. The perfection of the work is perceived the more it is studied, and it declares, in distinct language, the power and wisdom of the Author.

Lecture VI.—In pursuing the subject of the changes taking place in the solid parts of the surface of the globe, Mr. Davy devoted the principal part of this lecture to the consideration of the causes and effects of volcanos.

To persons who inhabit countries not liable to these phænomena, they appear rather as accidents than as essential events in the order of things: but their extent, their constancy, and their ultimate tendency, lead to a very different conclusion, and show that they are even necessary and useful in the great series of the phænomena of nature.

Volcanos have been active in all times, and in all quarters of the globe. A considerable part of Italy, of Sicily, and of the South of France, is volcanic.—The Cordilleras of South America, the highest mountains of the world, abound in these fires; and in Iceland, and the Asiatic Archipelagos, their effects are constant.

The striking circumstances in all the great volcanic eruptions are, said Mr. Davy, great productions of elastic matter, by which the ground is shaken and rent asunder, and earthquakes produced, and the pouring forth of a fused and ignited mass consisting of the earths in intimate combination.

Some persons have attempted to account for volcanic fires by supposing a central fire in the interior of the globe: but this notion, says Mr. Davy, cannot well be supported; for

for heat is communicable, and the surface in the course of ages would have gained the same temperature.

We can reason only by analogy, from known concerning unknown phænomena. Fire on the surface of the globe is usually a result of chemical changes: it is therefore reasonable to infer that subterranean fires depend upon similar causes; and the idea is supported by their cessation, renovation, and varied duration.

But what are the agents concerned in these great and awful elevations? The discoveries of Mr. Davy prove that the earths and alkalies consist of metals united to oxygen, or pure air; and these metals are highly inflammable, some of them so much so as to burn even in contact with water. The mean density of the earth, as determined by Mr. Cavendish and Dr. Maskelyne, would lead to the conclusion that the interior consists principally of metallic matter, which may be alloys of the metals of the earths and alkalies with the common metals:—and such an assumption, says Mr. Davy, would offer a ready explanation of subterranean heat and volcanic explosions; for, supposing water from the sea or lakes to act upon these inflammable masses, elastic matters would be rapidly disengaged, the surface would be broken, air would act upon the metals, inflammation would take place, and the result would be lava, the metals of the earths combined with oxygen.

Even a general hypothesis in Geology might be connected with the same idea. Rocks are decomposed and degraded by water, assisted by heat and atmospheric electricity. By the operations of volcanos, land is raised and matter newly consolidated; and it is possible to conceive electrical currents in the globe, by which metallic matter may be separated from oxygen, so as to preserve a perfect equilibrium between all the parts of the system,—an order in nature which may be conceived to be represented by the ancient hieroglyphic of the Phœnix rising from her ashes.

The reasonings derived from the consideration of volcanic fires relate to the future order of things, but cannot with propriety be applied to the formation of primary and secondary rocks. As yet no lavas have been found analogous to granite or porphyry; and though difference of pressure may interfere, yet this should not be assumed, but proved by experiments.

The obvious and immediate effect of volcanic fires is to increase the extent of the surface of the globe, and to raise land from the sea, and the soils produced from the decomposition are generally very fertile.—Some of the most beau-

 tiful

tiful of the islands in the Mediterranean seem to have been produced in this way; and the fertile soil of the Neapolitan territory, of the Azores, and of some of the Greek islands, is entirely volcanic.

Mr. Davy concluded by some observations on the general harmony and beauty of the laws of the system of the globe, attesting the wisdom and power of the Deity. The evil produced by volcanic eruptions is transient, the good permanent. The lava which destroyed Herculaneum has been for fifteen centuries a rich and fertile soil. The ashes which buried Pompeia have rendered a great country continually productive. The destruction is small and partial—the benefit great and general. In nature nothing must be judged of in moments, or from its immediate effects. Her operations are in years and in ages, and the ultimate tendency the preservation of life. It is by events apparently destructive that her powers are renovated. By her most awful and terrible agencies, even the equilibrium of things is restored—age and decay are prevented, and she is preserved in eternal youth.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society on the 27th of April, professor Jameson read a paper concerning the geognostic relations of the Iceland crystal. The Secretary communicated an account of the Colymbus Immer, or Ember-Goose, by Dr. Edmonston, of Shetland. And Dr. Gordon read an interesting paper, consisting of observations and experiments on the qualities of sensations of sound; on the different modes in which sonorous vibrations are communicated to the auditory nerve; on the ideas of the distance, and of the angular position of sounding bodies with respect to the ear, which are associated, by experience, with the different qualities of sounds; and on some of the more remarkable differences, in the *sense of hearing*, both original and accidental, which are occasionally observed among individuals, and, in particular, on the musical ear.

DUBLIN SOCIETY.

A letter from Mr. Thomas Harpur, of Moy, near Armagh, was laid before the Dublin Society on Thursday the 6th of May, stating, that he had invented a machine for working eight pumps of any length, of eight inches in the bore, and eighteen inches plunge, and to strike between thirty and forty strokes a minute each pump, with the easy labour of one man. The pumps can be set so close toge-

ther that they will take up less room than any other kind of pumps, and will raise more water on board ships, or on land, than can be raised in the same space of time by any chain-pump, (which requires 70 men to work them on board a man of war,) and will also raise more water than any steam-engine can do in the same time. The construction of the machine is simple, but powerful, and as quick in motion as the working pumps will admit of, and can be set up at a small expense. The letter further points out the great utility of such machines, particularly on board ships of all descriptions, in breweries, in distilleries, and for many other purposes. This machine is wrought by a rotatory motion, with easy labour for one man.

The same person has invented a churning machine worked by rotatory motion also.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Dobson, of the town and county of Newcastle-upon-Tyne, upholsterer, for certain improvements in the manufacture of rudder-bands and bolts for shipping.—May 1.

To John Moore, of Newington Causeway, in the county of Surry, lace-net-manufacturer, for a machine or machines for the manufacture of gold and silver twist, silk, cotton, or thread, twisted lace-net, similar to and resembling the Buckinghamshire and Northamptonshire lace as made by the hand with bobbins on pillows, and for making iron, brass, or copper wire-net.—May 1.

To John Ball, of Hethersett, in the county of Norfolk, engineer, for his improved cooking stove.—May 7.

To Thomas Cranfield, of Ilminster, in the county of Somerset, for his improvement upon machines for spinning and roving of cotton, flax, tow, hemp, wool, and silk, and twisting of thread.—May 7.

To Thomas Jones, of Oxendon-street, Piccadilly, in the county of Middlesex, mathematical instrument-maker, for his new instrument for dividing lines and distances, which will be useful to mathematicians, architects, and draftsmen. —May 9.

To Griffin Hawkins, of Water-lane, Tower-street, in the city of London, ship- and insurance-broker, for his apparatus calculated for the better defence of ships and vessels of different descriptions against being boarded or taken possession of by an enemy.—May 9.

To William Gilpin, of Wedges Mills, near Litchfield, in the county of Stafford, auger-maker, for his improved method of manufacturing augers.—May 16.

To

To John Street, of Hillfield Place, Clifton, in the county of Gloucester, esq. for improvements in the mode of making and working of bellows.—May 21.

To William Jenkins, of Birmingham, in the county of Warwick, brass-founder, for his improvement in the method of manufacturing flat backed handles and rings of different shapes and forms, used with or affixed to cabinet and other furniture and things, whereby much labour and expense in the manufacturing thereof are saved.—May 21.

To James Parsons, of Wellington, in the county of Somerset, builder, for his improved hinges and pulleys for doors and windows.—May 21.

To John Dickinson, of Ludgate Hill, in the city of London, stationer, for improvements in his patent machinery for making, cutting, and placing paper.—May 21.

To David Brewster, of Edinburgh, North Britain, doctor of laws, and William Harris, of Holborn, in the county of Middlesex, optician, for their optical instruments for measuring angles; and also certain instruments upon and additions to telescopes and other optical instruments for the purpose of measuring angles and distances with facility, and for other purposes.—May 21.

To George Gilpin, of Sheffield, in the county of York, worsted-spinner, for his machine or instrument for combing wool and preparing it for spinning, and also for dressing flax and preparing it for spinning; and also certain improvements in the construction of a machine known by the name of a Breaking Frame for drawing and clearing of the wool from the combs used in the first-mentioned machine, and also a stove to be heated by fire or steam for the purpose of heating the said combs.—June 11.

To Joseph Taite, of Bermondsey New Road, in the county of Surry, gent., Bryan Donkin, of Bermondsey, engineer, and William Dixon, of Bermondsey aforesaid, millwright, for their machinery for finishing piece goods or other flexible articles or materials of the like description, by glazing, burnishing, graining, or making impressions upon the surfaces thereof respectively, as may be required.—June 11.

To William Piper, of Woolverley, in the county of Worcester, iron-manufacturer, for his improved mode of manufacturing gun skelps.—June 11.

To Richard Waters, of Fore-street, Lambeth, in the county of Surry, potter, for his new method of manufacturing pottery ware.—June 14.

METEREO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For June 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	66	74°	64°	29·89	55	Fair
28	66	67	56	·70	48	Fair
29	50	69	49	·80	46	Fair
30	56	69	59	·93	70	Fair
31	60	66	57	·60	39	Showery
June 1	60	68	58	·70	51	Fair
2	59	60	56	·50	0	Rain
3	57	66	51	·90	46	Fair
4	58	64	54	·80	47	Fair
5	59	66	55	·82	59	Fair
6	62	67	54	·78	60	Fair
7	62	70	56	·98	66	Fair
8	66	78	54	·78	52	Fair, with thunder in the even.
9	62	69	53	30·01	46	Fair
10	60	78	56	·04	56	Fair
11	59	66	57	29·92	48	Fair
12	60	49	50	·91	65	Fair
13	56	65	49	30·11	70	Fair
14	58	68	55	29·92	72	Fair
15	62	74	58	·90	69	Fair
16	61	72	59	·98	74	Fair
17	60	69	60	30·25	64	Fair
18	61	70	61	·35	63	Fair
19	62	74	59	·12	84	Fair
20	57	58	50	29·83	35	Cloudy
21	50	60	45	·73	59	Fair
22	47	60	50	·89	46	Fair
23	54	60	54	·85	34	Cloudy
24	54	66	60	·68	26	Showery
25	61	70	59	·82	75	Fair
26	60	76	59	·90	88	Fair

N. B. The Barometer's height is taken at one o'clock.

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END OF THE THIRTY-SEVENTH VOLUME.

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Fig. 1

Fig. 2

Fig. 3.

Scale of 0 1 2 Inches

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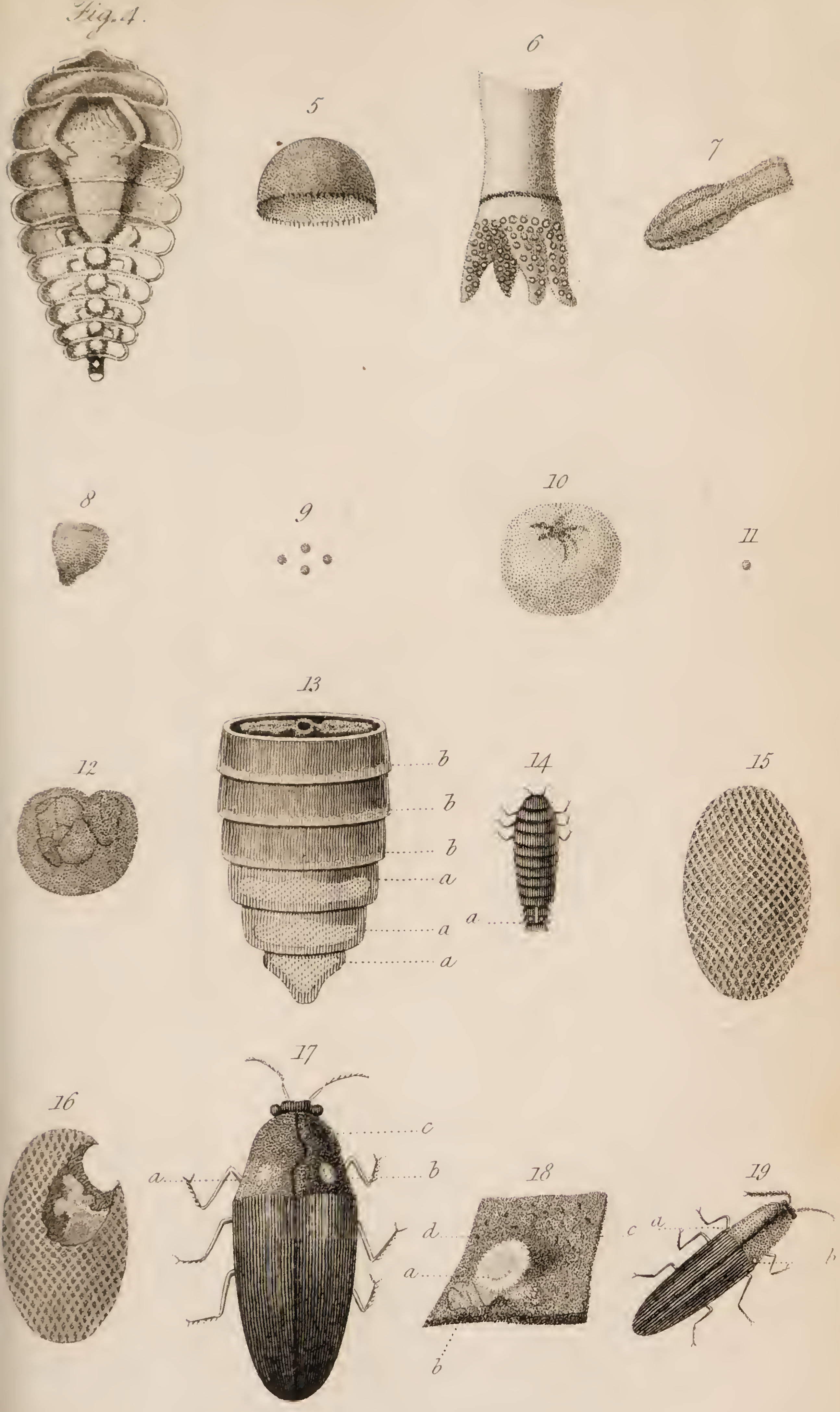
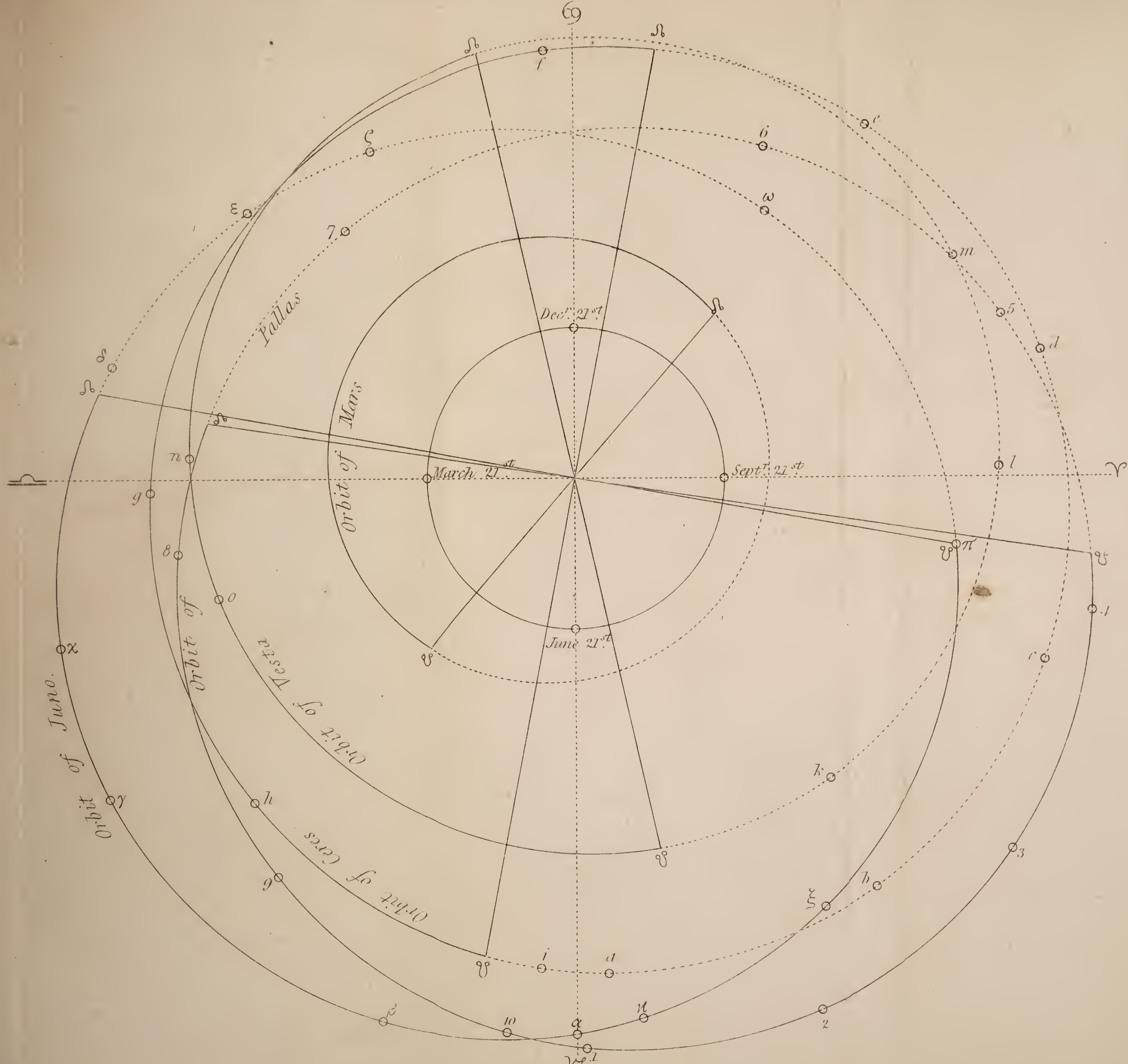
S. Porter sc.



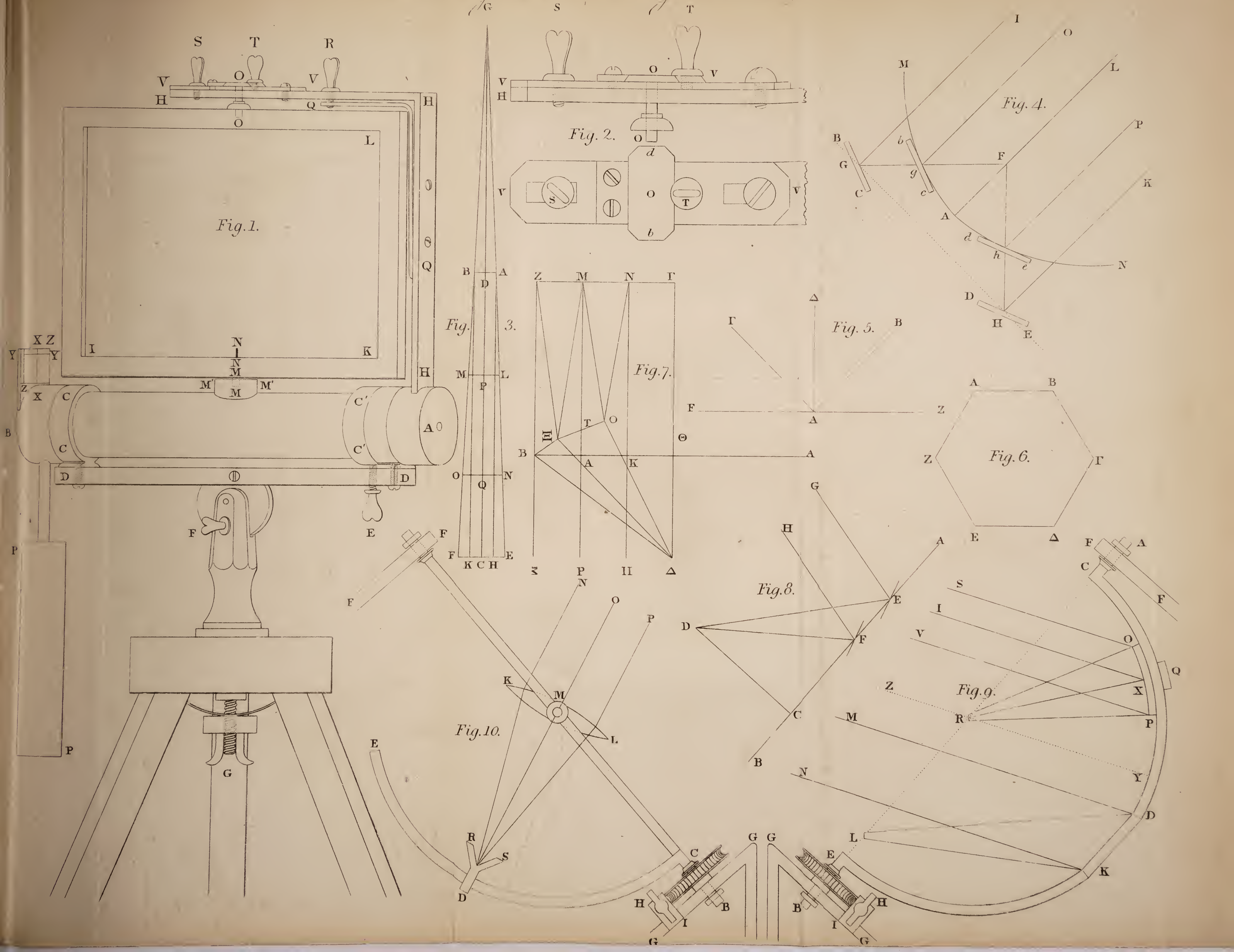
S. Porter sc.

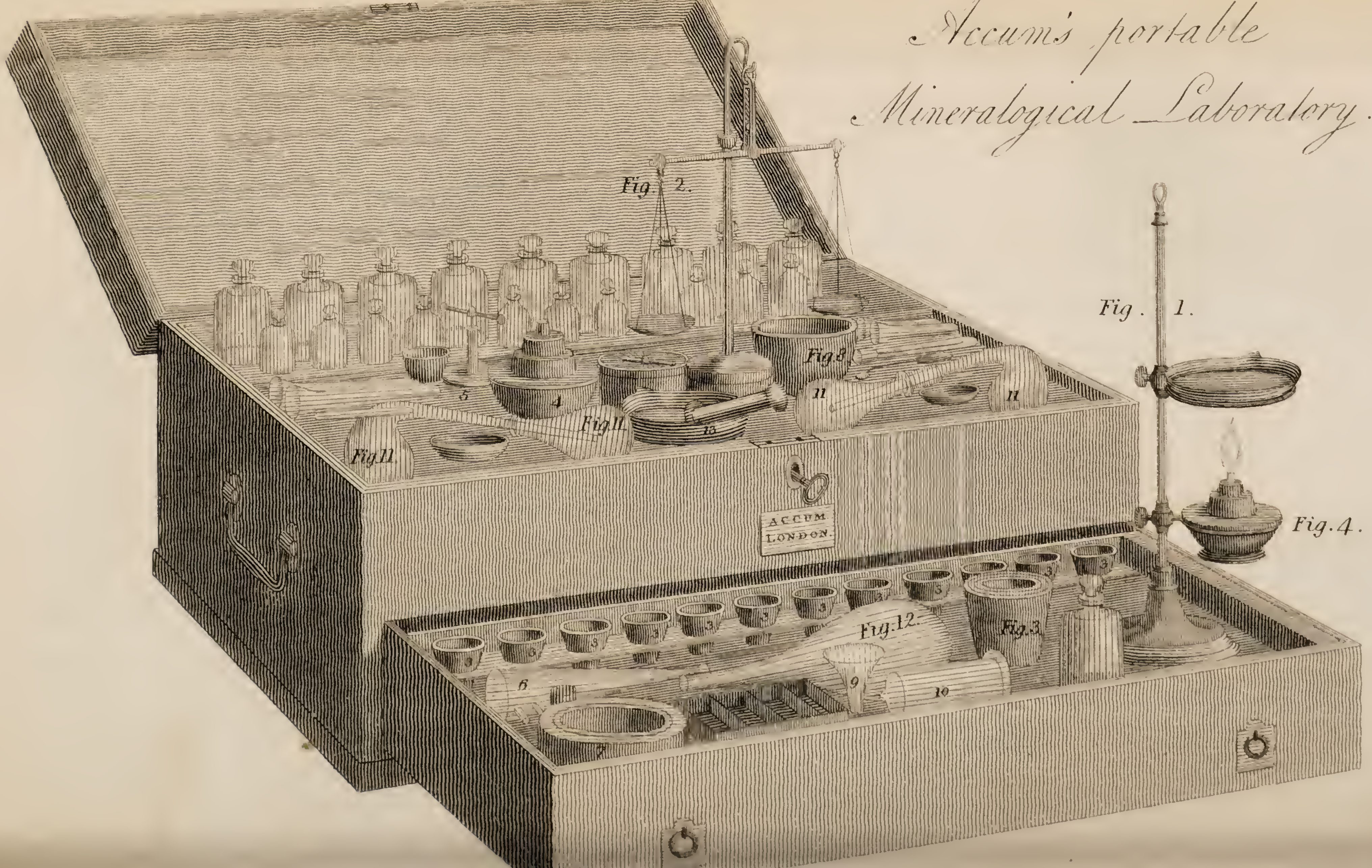
153.

Orbits of the Planets Ceres, Vesta, Pallas, and Juno.



M. Peyrard's Burning Mirror.





Accum's portable Mineralogical Laboratory.

Apparatus for the oxalate of Lime.

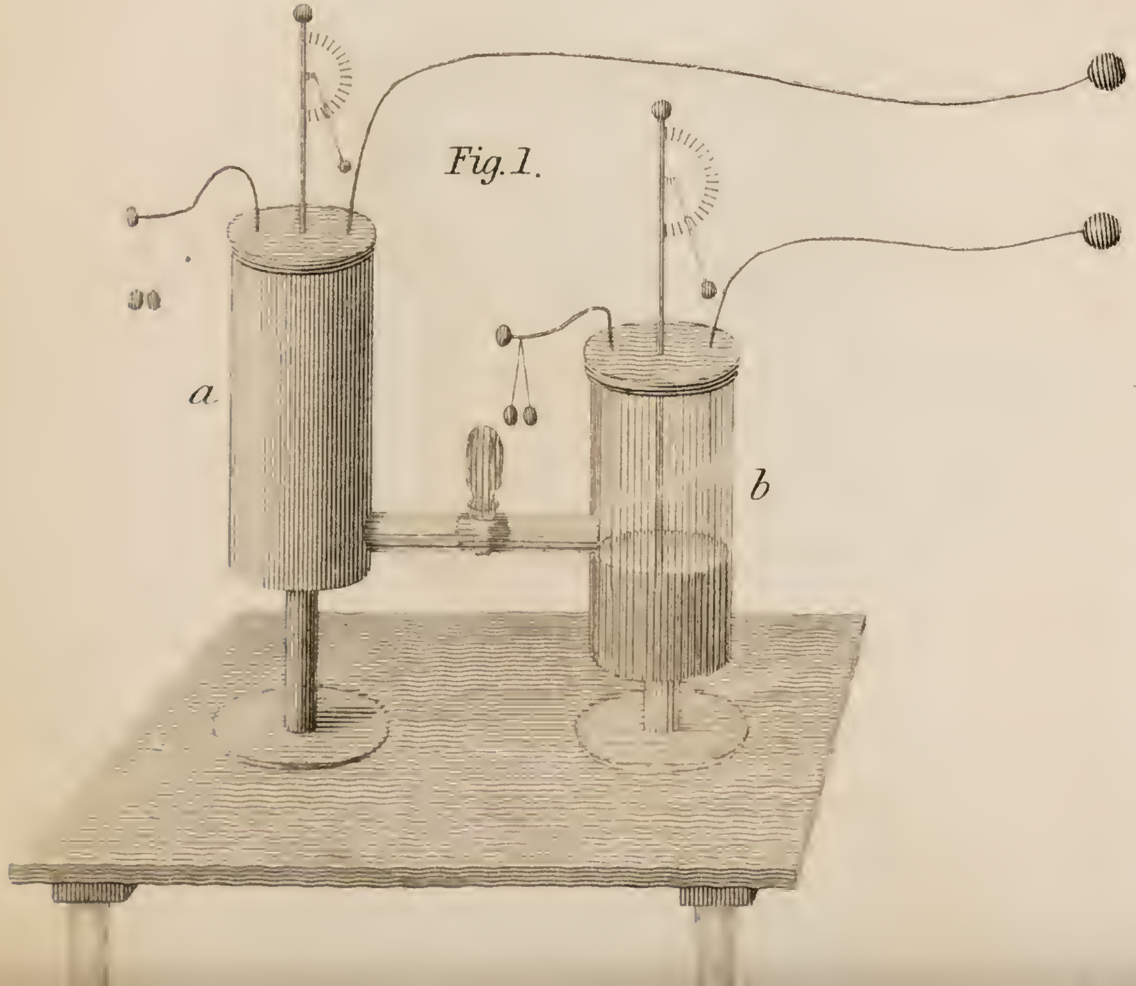


Fig. 1.

Fig. 3.

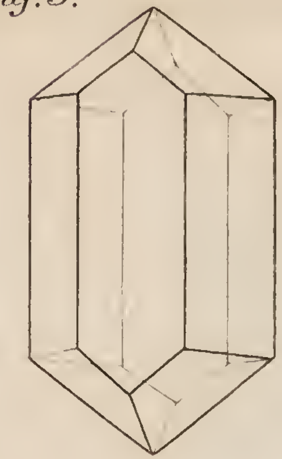


Fig. 4.

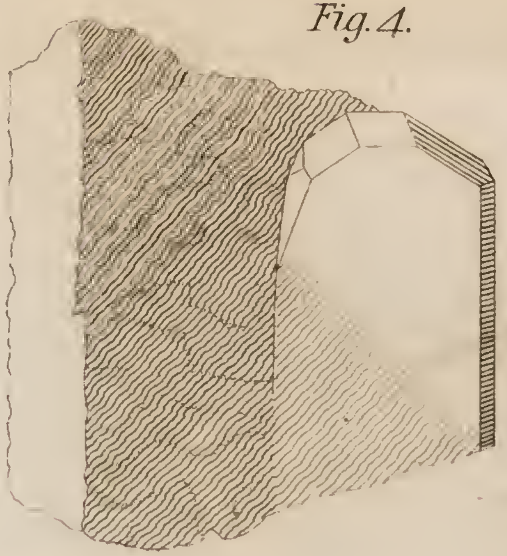
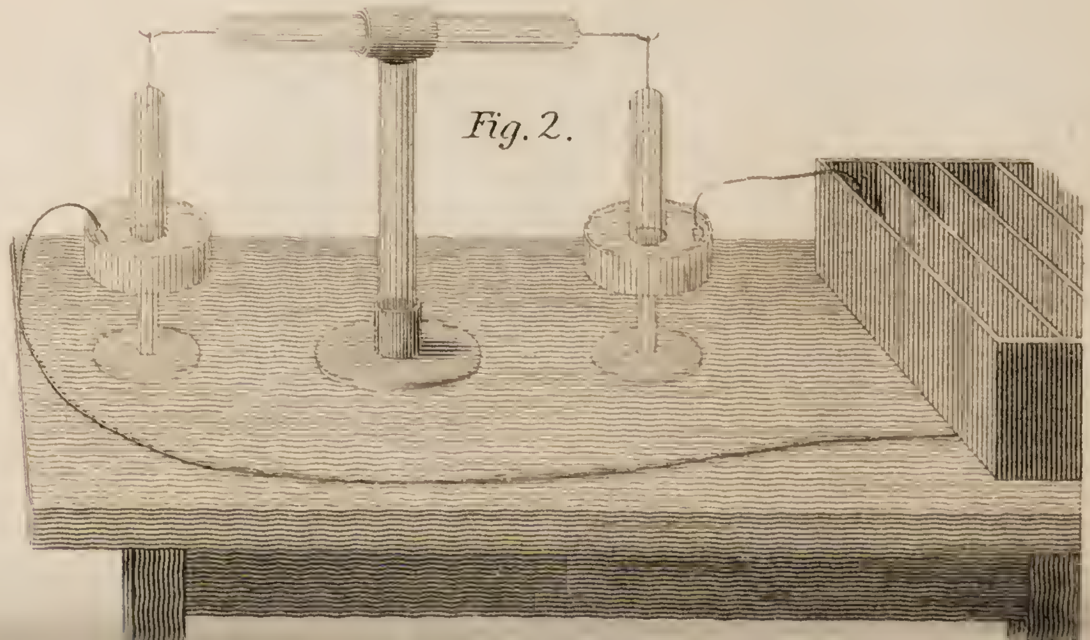


Fig. 2.



Mr. Lee's Thrashing Machine.

Fig. 1.

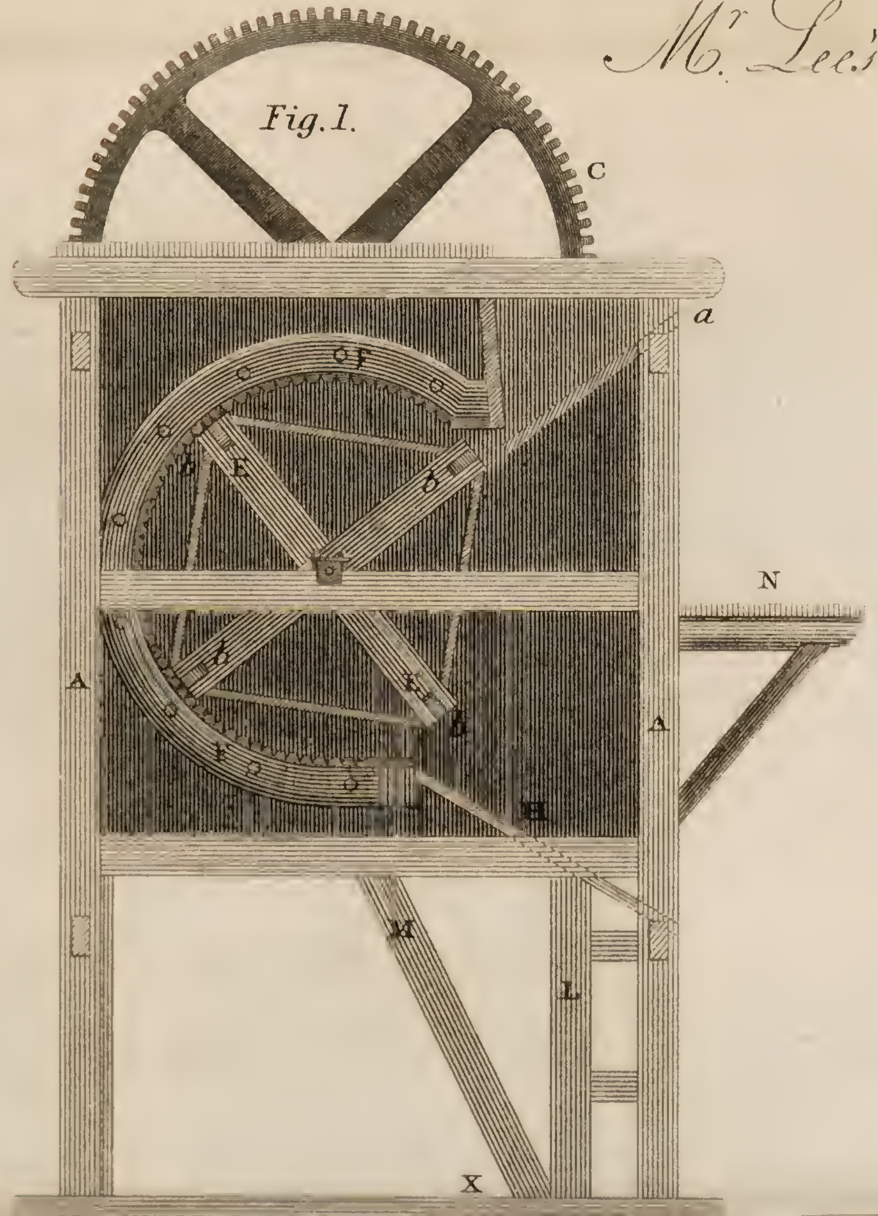
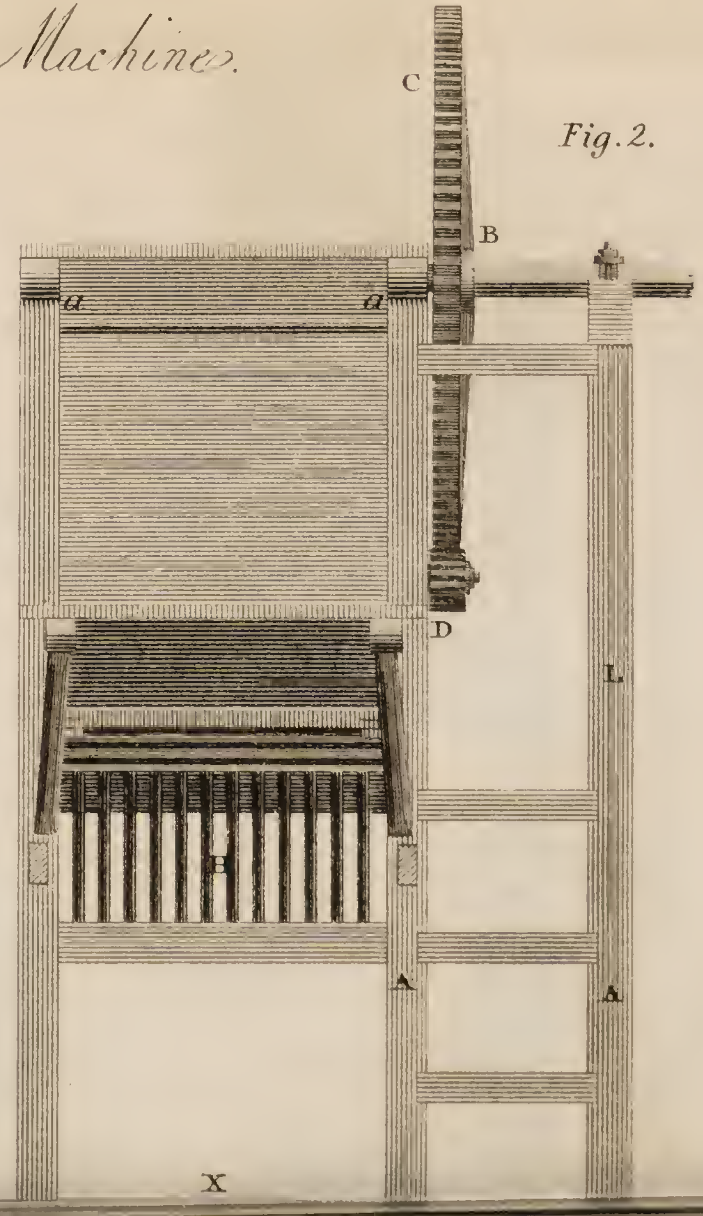


Fig. 2.



Mr. Cooke's apparatus for making gas, & other products from Pit Coal.

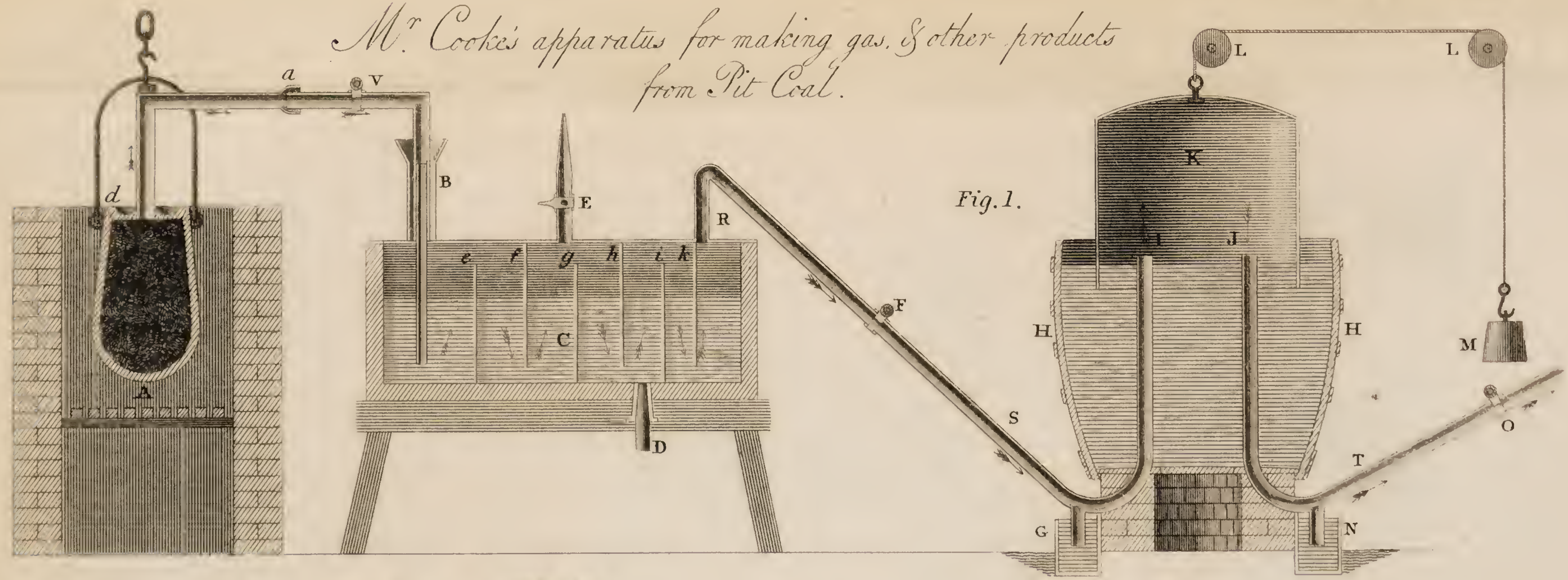


Fig. 1.

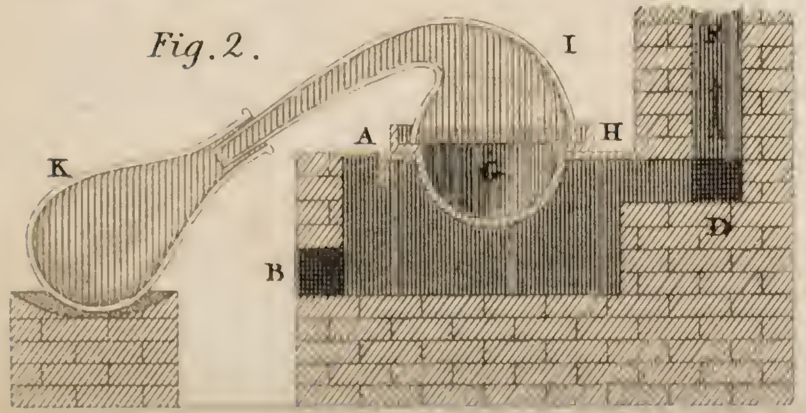


Fig. 2.

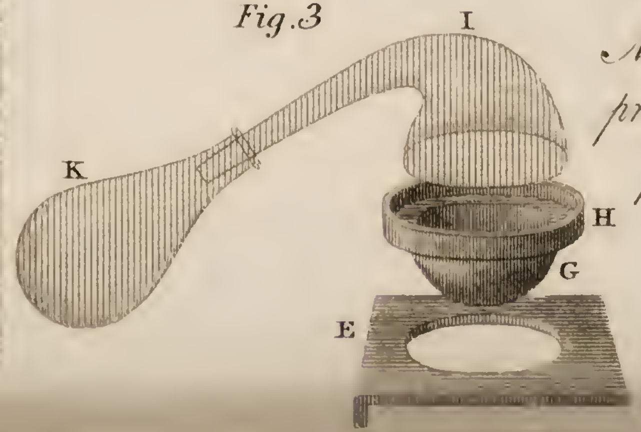
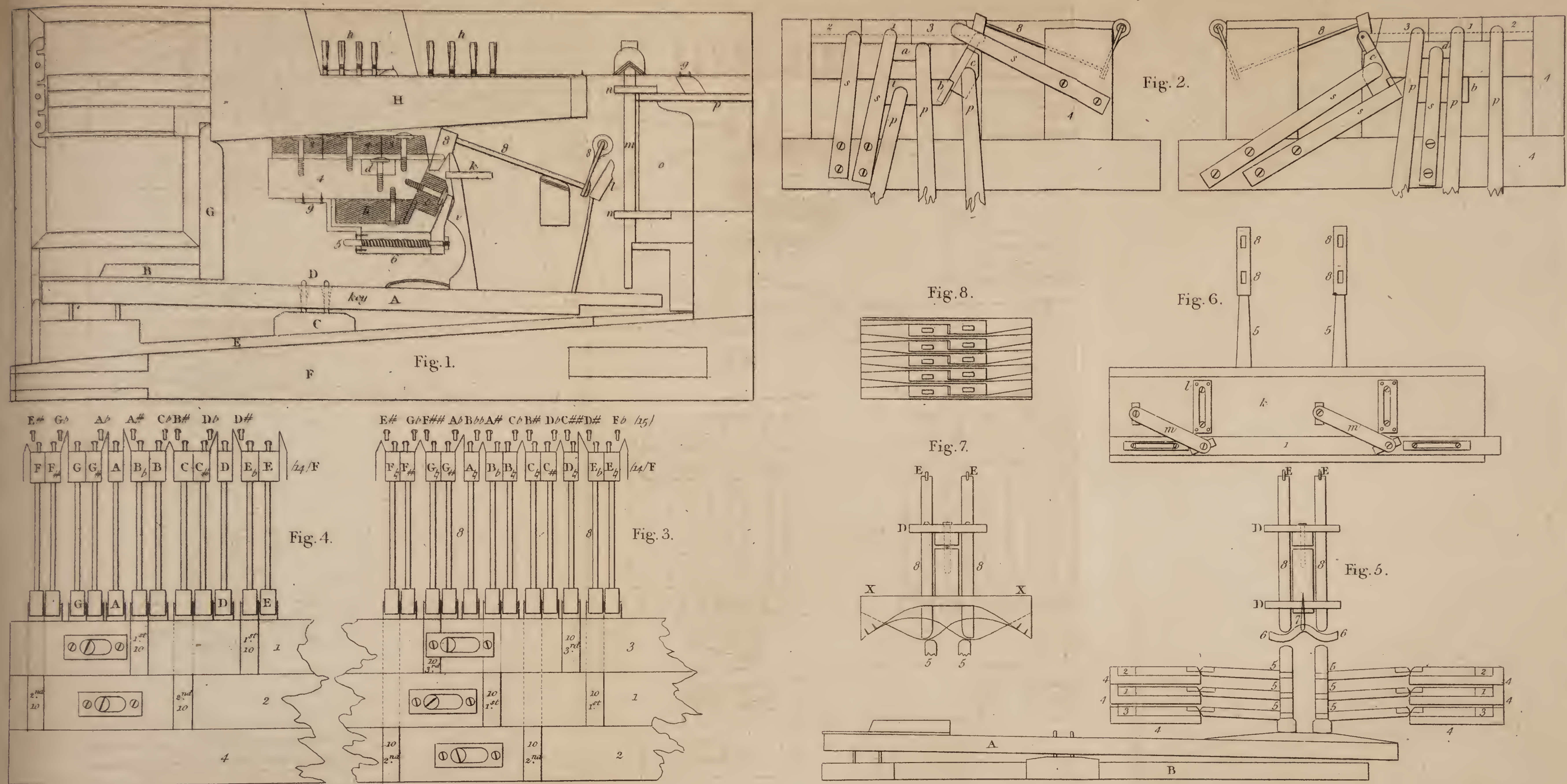


Fig. 3

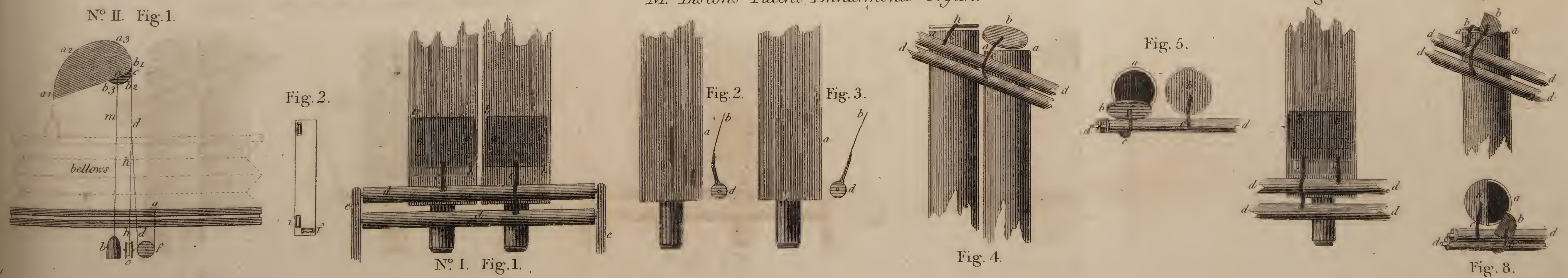
Mr. Way's method of procuring Turpentine from Fir Trees.

Fig. 4.

Mr. Loeschman's Patent Piano Forte.



Mr. Liston's Patent Enharmonic Organ.



J. Farey junr. delin.

S. Porter sculp.